MEDICAL REPOSITORY.

VOL. IV.—No. II.

ARTICLE I.

FACTS *relative to the* BLACK-VOMIT, DYSENTERY, *&c. as they occurred in Mifflin County, Pennſylvania, during the hot Weather of* 1797, 1798, *and* 1799, *and to the Efficacy of Strata of Calcareous Earth (Carbonate of Lime) in counteracting the exciting Cauſes of thoſe Diſtempers. From a Letter of Dr.* WILLIAM HARRIS *to Dr.* MITCHILL, *dated June* 24, 1800.

FIRST, I preſume, a brief deſcription of this country may be neceſſary. It lies to the north-weſt of Philadelphia nearly 200 miles: the ſoil is very fertile, and the country, in general, abounds in lime-ſtone, particularly the valleys. It is very mountainous. From within half or three-fourths of a mile from the foot of each mountain, the ſoil is chiefly underlaid with lime-ſtone, which, in many places, appears to be a ſolid rock. The valleys, viz. Nittany and Penn's Valley, are from four to ſix miles in breadth, within the lime-ſtone tracts. Bald-Eagle Valley lies to the north of theſe, in which is but very little lime-ſtone, if any. The valley is low, and generally flat. Near to the Bald-Eagle Creek, which runs through the valley from weſt to eaſt, are many ponds of ſtagnant water, filled with trees, rubbiſh, and putrid ſubſtances; which, in the months of Auguſt and September, become ſo offenſive, in many places, that the ſmell can ſcarcely be ſupported. This ſtench has only been ſo great ſince the clearing of the land.

It is now 25 or 30 years ſince the firſt ſettling of the country. Notwithſtanding the ponds of water which, during the whole ſummer, have been kept full, the inhabitants have enjoyed good health till within theſe three years. The

water is now expoſed to the rays of the ſun, and becomes nearly dried up towards the latter end of ſummer. It is, in many places, covered over with a greeniſh ſcum, from whence ariſes a very unpleaſant ſmell. The conſequence is dyſentery, intermittents, and highly malignant febrile complaints, which, in ſome inſtances, prove mortal in 48 hours, and three days, having every ſymptom, as deſcribed by medical writers, of the peſtilential diſeaſe, or "yellow fever," of New-York and Philadelphia. In the year 1797, it was as fatal in Mileſborough, which lies upon the Bald-Eagle Creek, according to the number of inhabitants, as it was in Philadelphia in 1793. —A large pond of water ſtood within the neighbourhood of the town. The ſeaſon being very dry and warm, the water, which before was ſhaded by trees, was now expoſed to the ſun, and was, in a great meaſure, dried up. The pond abounded with putrefying vegetables, the ſtench of which proved very diſagreeable. Few in the town eſcaped ſickneſs of the moſt malignant kind, which continued through the months of Auguſt and September. The pond has ſince been drained off, and the inhabitants are as healthy as uſual.

The diſeaſe was uſhered in by chills, pain in the head, back and limbs, often with vomiting. In ſome the complexion was very florid. Many diſcharged from the ſtomach a dark ſubſtance, firſt reſembling tar, and afterwards coffee-grounds. The diſeaſe, in ſeveral inſtances, was lengthened out to four and five weeks, which, in ſome, at that period, terminated favourably by profuſe hæmorrhages from the anus, and, in two inſtances, from the vagina. They were deſtitute, in a great meaſure, of medical aid, relying chiefly upon domeſtic preſcriptions.—The above ſketch of the diſeaſe I had from the moſt reſpectable inhabitants of the town.

In the year 1798 I removed to this country, when the ſame diſeaſe prevailed. The ſymptoms were, firſt, chills, pains in different parts of the body, particularly the head and back, ſucceeded by febrile commotions; the pulſe ſometimes tenſe, hard and full, often weak and frequent, always varying; the tongue, in moſt inſtances, was, from the commencement of febrile action, covered over with a thick brown or blackiſh fur. After the firſt 24 hours the ſick ſeldom ever complained of pain. They were often in a ſtate of ſtupor, with great inclination to ſleep. Delirium attended in almoſt every inſtance. Strangury was no unfrequent ſymptom. Singultus and hiccup were moſt commonly the fore-runners of diſſolution. Many, when aſked how they were, would ſay they felt no pain nor ſickneſs,

and would converſe freely, even a few minutes before death. Many vomited a dark, filthy, brown ſubſtance, and ſometimes nearly as dark as ink, which was ſo offenſive as to cauſe nauſea, and even vomiting, in the attendants. The fæces were, in ſome inſtances, of a very dark colour, and as offenſive to the ſmell. The nervous ſyſtem was much agitated. The limbs of ſome were in motion, as if performing feats of geſture; ſo violent was the ſubſultus tendinum. The ſkin was often very yellow. The linen was frequently ſtained with an orange tinge. In this year, emetics of tartarized antimony, cathartics of jalap and calomel, had a good effect. Bleeding was alſo very eſſential. The bark anſwered a good purpoſe; but, above all, alkalies were more congenial and ſerviceable.

The year 1799 was very dry, and the complaint more mortal, in general, than the two preceding ſeaſons. Within the neighbourhood of the ſame water, the inhabitants were viſited with the ſame diſeaſe, in its higheſt degree of malignancy. Dyſentery and intermittents alſo prevailed; but, as far as I recollect, none were affected with them except thoſe who lived in the neighbourhood of, or near to, the ſource of the miaſma. Several who viſited the ſick inhaled the poiſon; and, after returning home, ſickened and died. It was in no one inſtance contagious. The mercury in the thermometer ſtood often as high as 95 deg. and, indeed, one day, in the ſhade, at 99 deg. The degree of heat was very variable.—This ſeaſon the ſymptoms were much the ſame as the preceding, only more violent. The black-vomiting was characteriſtic.

One circumſtance I think worthy of attention, which plainly evinces the unhealthineſs of putrid vegetables, &c. when expoſed to the ſun.—A large ſpring iſſued from the bottom of a ſand-ſtone hill, ſufficiently large to turn a ſaw-mill, which had always, from the firſt tillage of the country, been a conſtant and uniform ſtream, until the preſent autumn, when, after a very dry ſummer, it ſtopt running. A houſe was built cloſe upon the bank of the ſpring, which contained ſeveral families, who always, heretofore, had been very healthy. The ſtream from it was full of filthy rubbiſh, which, when dried up, had a very diſagreeable ſmell. The families which lived in the houſe were taken ſick with a diſeaſe reſembling that of 1797, of which, after a few days ſickneſs, three died: the remainder continued ſickly and confined till the water returned: they then recovered. Several of their friends, who came 20 and 30 miles to viſit them, took ſick, and died upon their return, without ſpreading the complaint.

Several, this ſeaſon, had dyſentery and intermittents at the ſame time: the one always yielding to its ſuperior in the courſe of a few days.

In the lime-ſtone tract, I do not know of an inſtance of the diſeaſe occurring, but what could be traced to ſome unhealthy part, near to the foot of the mountains. In Nittany Valley, between the borders of the lime-ſtone and that of the mountain, cynanche trachealis and dyſentery were frequent, and ſome intermittents. It puzzled me to know the reaſon of the remainder of the valley being ſo healthy, and thoſe narrow confines ſo unhealthy, though it is true there is more moiſture in the latter. Rheumatiſms and pneumonic affections are the moſt prevailing complaints in the middle of this valley. Emetics, ſucceeded by alkaline ſubſtances, I found to do better than the lancet, and other uſual remedies. Cathartics of calomel were very beneficial, and bark anſwered alſo this ſeaſon a good end. The lancet, though my uſual friend in fever, was, in this inſtance, undoubtedly a foe to health. I laid it aſide, and depended chiefly upon the above for deſtroying the poiſon of the prevailing diſeaſe. When timely adminiſtered, it ſucceeded.

I am, with ſentiments of reſpect,
Yours, &c.

WILLIAM HARRIS.

ARTICLE II.

On the EFFICACY *of the* DIGITALIS PURPUREA *in* ALLAYING EXCESSIVE ACTION *of the* SANGUIFEROUS SYSTEM: *Communicated to Dr.* MITCHILL *by* JAMES S. STRINGHAM, *M. D. of New-York.*

THE latter end of the month of April laſt I was called upon to viſit a gentleman in this city, who was labouring under a ſevere attack of pneumonia. When I ſaw him (which was on the third day of the diſeaſe), his face was highly fluſhed, the tunica albuginea extremely turgid. About two hours before I came he had been ſeized with delirium; complained of great pain, particularly on the right ſide, ſhooting from thence to the ſcapula; his ſkin was hot and dry; pulſe about 120; reſpiration laborious, attended with a ſhort dry

cough. The uſual remedies had been applied by the phyſician who firſt attended him; but they had contributed little to his relief. The obſtinacy and violence of the fever made me determine, if poſſible, to adminiſter ſome remedy which might, by its firſt and immediate operation, diminiſh the action of the arterial ſyſtem. Of thoſe contained in the materia medica, I formed the moſt favourable expectations from the digitalis, as I thought that in one caſe, where it had been taken by miſtake to the quantity of three grains, it had, in this way, greatly aſſiſted in the cure of an incipient enteritis, though I, at that time, attributed its good effects not to any action peculiar to the digitalis, but to the great nauſea and vomiting it occaſioned, which terminated in a profuſe ſweat. I, conſequently, thought that it might properly be adminiſtered with a twofold view. Firſt, I hoped that by giving it in ſmall doſes, and at ſhort intervals, my patient might be able to retain a ſufficient quantity of it on his ſtomach to act quickly and generally on the ſyſtem. If I was diſappointed here, and ſhould nauſea or vomiting be produced, I hoped that theſe might be ſucceeded by that evacuation which had proved ſo ſalutary in the caſe juſt alluded to. Accordingly, at ten o'clock on the ſame morning in which I was firſt called, I began by ordering him to take half a grain every three hours, unleſs he found his ſtomach or head affected by it. At ſeven in the evening I viſited him again, and found the quickneſs of pulſe greatly diminiſhed; and his nurſe remarked to me, that he had voided more urine that day than on any other during his illneſs. He had had no delirium during the laſt ſix hours; nor was that turgidity of the countenance by any means ſo remarkable as in the morning.

He had now taken two grains of the digitalis. Encouraged by a change ſo flattering (as the man was of a ſtrong and robuſt conſtitution), I determined, by puſhing my medicine, to give it a fair trial. I therefore ordered a continuance of it, with the ſame proviſo as formerly. Feeling ſome anxiety as to the event of my experiment, and not placing the greateſt confidence in the attention and punctuality of the nurſe, I viſited him again at nine. He had had another profuſe diſcharge of urine ſince I ſaw him laſt; and every appearance was ſo much changed for the better, that I did not heſitate to pronounce him out of danger. His pulſe now did not exceed ninety ſtrokes in a minute. I was preſent at his taking the fifth doſe, which, ſhortly after it was received into the ſtomach, produced what he termed an inſupportable nauſea. This was ſucceeded by

vomiting, which not being very ſevere, I encouraged by diluents. After this the medicine operated briſkly as a cathartic. I continued with him for ſome time; and, after ordering a diſcontinuance of the digitalis, I left him compoſed, and with an incipient moiſture upon the ſkin. The next morning I found that he ſtill continued convaleſcent. On inquiring whether he had ſince felt any diſagreeable effects from his medicine during the night, he aſſured me that he had not, except an uncomfortable degree of ſweating. On the third day of my viſiting him, I obſerved that with his cough he had ſome expectoration. I now gave him the oxymel ſcilliticum, at the ſame time paying attention to the ſtate of his bowels. Upon this plan of treatment, in a ſhort time, he ſo far recovered as to render my further attendance unneceſſary.

Shortly after the above trial, a ſchool-maſter in this city conſulted me about a fixed and obſtinate pain in his breaſt, which he had experienced at intervals during ſeveral years, firſt occaſioned, as he ſuppoſed, by a too intenſe application to writing. It may be proper to remark, by the way, that in this caſe there was no cough. After taking ſome blood from him, and opening his bowels, I applied bliſters ſucceſſively to the painful part. To theſe was alſo added the uſe of diaphoretics, and an antiphlogiſtic regimen, but all without any evidently good effect. I determined, in this caſe alſo, to try the virtues of the digitalis. He began with half a grain night and morning, which, after a few days, was increaſed to a grain. He continued perſevering in this plan about a fortnight, when he aſſured me that he found himſelf greatly better, and wiſhed to know whether it was neceſſary to continue the medicine. This I ſtrongly recommended; notwithſtanding which, his neglect of remedies (as commonly happens) increaſing in proportion as his complaint diminiſhed, the pain again returned with its former ſeverity. I again put him on the ſame plan, with the ſame ſucceſs—as he now aſſures me that it does not prevent him from proſecuting the duties of his ſtation with eaſe and ſatisfaction.

I have often thought, that a medicine which would diminiſh the action of the vaſcular ſyſtem, without previouſly occaſioning an increaſed excitement, was a great deſideratum amongſt phyſicians. The caſes juſt now related induce me to hope that this want may be, at leaſt in ſome degree, ſupplied by the digitalis, as its firſt effect (ſo far as I could obſerve)

after its exhibition, was that of rendering the pulſe ſlower. With this circumſtance, I alſo apprehend, are intimately connected that increaſe in the quantity of urine and ſubſequent perſpiration, which ſeemed, in ſo ſhort a time, to place my firſt patient entirely out of danger. I have ever conſidered a too rapid circulation of the fluids equally unfavourable to perſpiration and ſecretion with that which is too tardy and languid. When the blood is thus precipitately hurried away, a ſufficient time is not given for the ſeparation of its thinner parts, deſtined to enter either the ſmall and delicate excretories which open on the ſurface, or thoſe ſtill more minute ramifications which conſtitute the glands.

I have been particular in remarking the diuretic effects of the digitalis on my firſt patient, and that the firſt favourable change in his diſeaſe was preceded by an increaſed flow of urine. In febrile complaints, the ſtate of the urinary ſecretions is, in my opinion, at preſent, too little regarded by practitioners. If (as is generally taught) fevers are produced and continued by an obſtruction or ſpaſmodic contraction of the extreme veſſels, by which fluids are pent up in the body, which prove noxious both by their quantity and acrimony, does it not appear rational to ſuppoſe, that a diſcharge of ſuch fluid from the ſyſtem, by means of any other organ, muſt greatly aid in the cure of the diſeaſe—*Ceſſante cauſa ceſſat effectus.* Urine, and the matter of perſpiration, as to their properties, appear to have conſiderable ſimilitude; and we all know how much the ſtate of the one is affected by that of the other. Thus, in winter, to prevent the bad effects of diminiſhed perſpiration, the urine becomes more copious and aqueous. In ſummer, again, when the firſt is increaſed in quantity, the latter is more ſparingly diſcharged. Thoſe who uſe much exerciſe, by increaſing perſpiration, paſs much leſs urine than the ſedentary; whereas, diminiſhed perſpiration always produces a greater ſecretion from the urinary organs: hence the bladders of old men are much more frequently diſtended than thoſe of youth. From theſe, and many more inſtances that might be adduced, we perceive that there is an intimate connection between theſe two excretions, and that nature prevents the bad conſequences that might ariſe from a diminution of the one, by an augmentation of the other. Ought not ſhe, in this reſpect, to be imitated by phyſicians? I muſt confeſs, for my own part, that I am willing to attribute much of the good effects produced by the digitalis, in the cure of my patient, to its diuretic properties; and this opinion is corrobo-

rated by what has been juſt obſerved as to natural proceſſes for the ſame purpoſe. Hence I am induced to hope, that in this way alſo the digitalis may prove a valuable remedy in the cure of fevers. It may, at firſt ſight, appear ſomewhat ſingular, that one of theſe patients ſhould be able, in the courſe of a day, to take two and a half grains of this medicine, without ſuffering from its effects. But thoſe who are accuſtomed to the exhibition of powerful remedies in violent diſeaſes, well know that the conſtitution will then bear, without inconvenience, medicines which, in a natural and healthy ſtate, would produce the moſt pernicious conſequences. Mania and fevers, in particular, afford numerous proofs of the truth of this remark. This, perhaps, may be owing to that determination of fluids towards the cerebrum, by which the nerves become, in ſome degree, compreſſed at their origin: hence a diminiſhed ſenſibility of the ſyſtem.

I have ventured to propoſe to your conſideration the foregoing remarks, with a view to excite the attention of others on a ſubject which I deem highly intereſting. I regret that (being but a young practitioner) my opportunities for obſervation have been too contracted to form any deciſive opinion; I ſhall, however, make uſe of every mean for a further inveſtigation of the ſubject; and the reſult of theſe inquiries I ſhall candidly tranſmit to you.

New-York, July 11, 1800.

ARTICLE III.

OBSERVATIONS *on* CERTAIN OBJECTIONS *of Dr.* JOSEPH PRIESTLEY *to the* ANTIPHLOGISTIC SYSTEM *of* CHEMISTRY. *By* JAMES WOODHOUSE, *M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania.*

No. II.

To the Editors of the Medical Repoſitory.

GENTLEMEN,

IN my defence of the antiphlogiſtic theory of chemiſtry, publiſhed in the Medical Repoſitory, vol. iv. p. 25, I decidedly pronounced, that the ſcales which the blackſmiths ſtrike off from red-hot iron, called by Dr. Prieſtley finery cinder, were

compoſed partly of water, which is contrary to the belief of the French chemiſts, who conſider them as a combination of iron and oxygen, though I have attempted to reconcile my opinion with their theory.

Having lately performed a variety of intereſting experiments, I have obtained reſults which are as much, or rather more, in favour of Dr. Prieſtley than his opponents.

In inveſtigating a ſubject which requires the attention to be kept conſtantly awake, it is not improbable but that ſome miſtakes may be committed. Should any be made by myſelf, I ſhall always acknowledge them, for nothing is more deſirable than truth in ſcience. Following the laudable example of Dr. Prieſtley, I ſhall endeavour to imitate his well-known candour, and ſtrict adherence to matter of fact—

> " Non ita certandi cupidus, quam propter amorem
> " Quod eum imitari aveo." LUCRET.

FIRST. *Of the Calces of Metals and Coal expoſed to a red Heat, &c.*

The ſubſtance produced by tranſmitting the ſteam of water over red-hot iron, appears to conſiſt of the ſame principles as the ſcales of iron; for, when mixed with coal which has ceaſed to give air, it yields large quantities of carbonated inflammable and fixed air. The celebrated Lavoiſier, the founder of the new ſyſtem of chemiſtry, who is erroneouſly ſuppoſed to have experimented with more accuracy than any other perſon, was of opinion, that the oxygen of the water united to the iron, and that the increaſed weight of the metal was owing purely to oxygen, and to no other cauſe. In this he has been followed by all who have embraced the new doctrine, and the quantity of oxygen contained in water has been deduced from the increaſed weight of the iron.

Now, if it can be proved that part of the weight of finery cinder is owing to water, it will follow, that the calculation of the quantity of oxygen in water, from the increaſed weight of the iron, muſt have been groſſly erroneous.

The proofs that finery cinder contains water are two.

1ſt. If it was compoſed merely of oxygen and iron, it would yield nothing but fixed air when ſubjected to heat with pure coal, as red precipitate does, which is not the caſe.

2dly. When added to coal which has ceaſed to yield air,

it affords inflammable and fixed air in the ſame manner as if the ſteam of water was paſſed over hot coal, or if it was united to water, or any ſubſtance containing water.

Eight ounces of the ſcales of iron, and half an ounce of coal, yielded ſix hundred and two ounce meaſures of carbonated inflammable and fixed air. The iron was completely revived; and, by ſulphuric acid and water, gave common inflammable and hepatic gas. The laſt air conſiſts of ſulphur diſſolved in inflammable air, and ſulphur is probably a compound body, for it appears to have been formed during the experiment. The air changed the water of my hydropneumatic tub to a brown colour, turned ſilver black, and produced this colour on the ſhelf of the tub, which was painted white.

Having eſtabliſhed the proofs that finery cinder contains water, I ſhall endeavour to ſhow that oxygen is one of its component parts, which Dr. Prieſtley will not allow.

If there was nothing but water in the ſcales of iron, as much fixed air could be produced by coal mixed with water, or by paſſing ſteam over red-hot coal, or by adding water to ſome other ſubſtance not containing oxygen, and mixing it with coal, as from finery cinder and coal.

I have never been able to obtain more than thirty parts in the hundred of fixed air, from paſſing ſteam over hot coal, or by mixing it with water, and expoſing it to heat in an earthen retort; but with finery cinder and coal I have obtained fifty parts in the hundred of fixed air. I have paſſed from two to twenty ounce meaſures of water, in the form of ſteam, over half an ounce, and an ounce of red-hot coal, in moderate pieces, in a copper tube, and though I have three times examined the air, for two hours at a time, I never could find more than thirty parts in the hundred of fixed air.

The method of determining the quantity of fixed air in one hundred parts of inflammable and fixed air, is by receiving the airs over water, in the meaſure of an eudiometer, and immediately throwing them up over lime-water. The fixed air unites to the lime, an abſorption of the water takes place, and the proportion is determined by a ſcale accurately graduated.

By relating the experiments, at length, on coal alone, and mixed with finery cinder, the difference in the quantity of inflammable and fixed air will be made manifeſt.

Four ounces of coal, taken promiſcuouſly from a heap, yielded ſix hundred and twenty-two ounce meaſures of inflammable and fixed air.

The 1ſt	10 ounce meaſures was the air of the veſſel.		
2d	4	contained 30 fixed air and	70 inflam.
3d	4	20	80
4th	4	15	85
5th	360	10	90
6th	70	2	98
7th	170	0	100
	622		

The ſame coal, taken from the retort, wetted with water, and committed to it again, gave eighty-one ounce meaſures of inflammable and fixed air.

The 1ſt	10 ounce meaſures was the air of the veſſel.		
2d	12	contained 30 fixed air and	70 inflam.
3d	40	25	75
4th	6	20	80
5th	13	0	100
	81		

The ſame coal, wetted a third time with water, yielded one hundred and eighty-one ounce meaſures of inflammable and fixed air.

The 1ſt	14 ounce meaſures was the air of the veſſel.		
2d	4	contained 30 fixed air and	70 inflam.
3d	44	26	74
4th	37	25	75
5th	62	20	80
6th	12	8	92
7th	4	1	99
8th	4	0	100
	181		

I continued to add water to this coal, and expoſed it to heat, until it was conſumed: the proportion of fixed air always gradually diminiſhed—the firſt portions being 30, and the laſt 5, or purely carbonated inflammable air.

When the ſcales of iron were added to the coal, the proportion of fixed air was much greater.

Eight ounces of the ſcales of iron, and half an ounce of coal, which had ceaſed to yield air, expoſed to heat in an earthen retort, in five hours gave ſix hundred and two ounce meaſures of inflammable and fixed air.

The 1ſt	14	ounce meaſures of atmoſpheric, inflammable, and contained	30	fixed air.
2d	30		50	50 inflam.
3d	8		46	54
4th	8		45	55
5th	20		40	60
6th	18		36	64
7th	18		30	70
8th	84		25	75
9th	128		22	78
10th	96		22	78
11th	178		20	80
	602			

When two ounces of the ſcales of iron, and one drachm of coal, were uſed, the firſt portions of air were 40 in 100 fixed air: when half an ounce was tried, the fixed air was 30 per cent. and when two drachms of that made by throwing ſteam over red-hot iron wire was mixed with one drachm of coal, ſixty ounce meaſures of inflammable and fixed air were procured—the firſt portions of which were 20 per cent. fixed air, and the laſt 25.

The firſt thing which ſtrikes us, in theſe experiments, is the large quantity of pure carbonated inflammable air afforded by coal, taken promiſcuouſly from a heap, as it comes to us from the dealers in that article, compared to the quantity yielded by the ſame coal, wetted with water, and repeatedly expoſed to heat. Four ounces of this kind of coal gave 170 ounce meaſures of inflammable air, entirely free of fixed air; but when firſt wetted with water, the inflammable air was thirteen ounce meaſures, and the ſecond time four ounce meaſures.—One hundred and forty-three ounce meaſures of inflammable air was afforded by two ounces of another ſpecimen of coal; and, when wetted with water, the inflammable air was diminiſhed to two ounce meaſures.

I can only account for the fact by ſuppoſing, that common coal is imperfectly charred, and that the volatile principles of the wood are diſſipated, and conſiſt chiefly of inflammable air.

The gaſes which are procured from coal and water, have been aſcribed to the decompoſition of water. It is ſaid that the oxygen of the water unites to part of the coal, and forms the fixed air; while its hydrogen diſſolves another part of the coal, and forms carbonated inflammable air. But this theory will not account for the gradual increaſe of the inflammable,

and diminution of the fixed air, when coal is expoſed to heat. As water, according to the French chemiſts, is compoſed of eighty-five parts, by weight, of oxygen, and fifteen of hydrogen, the eighty-five parts of oxygen ought conſtantly to unite with the coal, and form fixed air, and the proportion of inflammable and fixed air ſhould invariably be the ſame; or where two parts, in bulk, of the inflammable air are found, there ought to be one part of oxygen united to the coal, which is not the caſe; for when the airs are obtained from coal, the firſt portions are generally 70 inflammable, and 30 fixed air; and the laſt are either all inflammable, or 95 inflammable, and 5 fixed air.

The ſame continued increaſe of inflammable air, and diminution of fixed air, takes place upon mixing the oxyds of zinc, iron, copper, lead, manganeſe, tin and biſmuth, with coal, as will be ſhown by the following experiments.

Half an ounce of zinc, precipitated from a ſolution of white vitriol by cauſtic pot-aſh, was waſhed in water until it would not give a milky colour to muriated barytes, and was expoſed to a red heat half an hour, and then mixed with two drachms of coal, which had ceaſed to yield air, in an earthen retort, when it gave eighty-ſix ounce meaſures of inflammable and fixed air.

The 1ſt	10	ounce meaſures was the air of the veſſel.	
2d	4	contained 75 fixed air	and 25 inflam.
3d	4	40	60
4th	4	15	85
5th	4	10	90
6th	4	2	98
7th	56	0	100
	86		

The ſame reſult happened from uſing the flowers of zinc and coal. The metal is completely revived, and is found adhering to the neck of the retort, which muſt be broken to obtain it. In three experiments, however, fifty, forty, and twenty-four ounce meaſures of pure carbonated inflammable air were procured without any perceptible quantity of fixed air.

One ounce of the red oxyd of iron, colcothar of vitriol, made by expoſing green vitriol to an intenſe heat, and one drachm of coal, gave ſeventy ounce meaſures of inflammable and fixed air; the firſt portions being wholly fixed air, then 80, 60, 50, and never leſs than 40 parts in the hundred.

Two drachms of the calx of iron, from a ſolution of green

vitriol by cauſtic pot-aſh, which had been half an hour expoſed to a red heat, and one drachm of coal, gave two hundred and ſixty-nine ounce meaſures of inflammable and fixed air.

The 1ſt	4 ounce meaſures	was the air of the veſſel.	
2d	4	contained 65 fixed air and	35 inflam.
3d	4	60	40
4th	4	42	58
5th	4	30	70
6th	5	25	75
7th	200	20	80
8th	44	15	85
	269		

Half an ounce of the oxyd of copper, from blue vitriol by cauſtic pot-aſh, which was expoſed half an hour to a red heat, and one drachm of coal, yielded one hundred and ſix ounce meaſures of inflammable and fixed air.

The 1ſt	4 ounce meaſures	was the air of the veſſel.	
2d	36	contained 100 fixed air and	0 inflam.
3d	10	74	26
4th	10	20	80
5th	8	12	88
6th	20	10	90
7th	18	5	95
	106		

Half an ounce of red lead, and one drachm of coal, gave twenty-ſix ounce meaſures of inflammable and fixed air.

The 1ſt	4 ounce meaſures	was the air of the veſſel.	
2d	10	contained 100 fixed air.	
3d	8	45	
4th	4	15	
	26		

Half an ounce of white lead, and one drachm of coal, afforded fifty-three ounce meaſures of inflammable and fixed air.

The 1ſt	4 ounce meaſures	was the air of the veſſel.	
2d	4	contained 80 fixed air and	20 inflam.
3d	18	95	5
4th	4	85	15
5th	9	70	30
6th	8	20	80
7th	6	10	90
	53		

Half an ounce of black oxyd of manganeſe, and one drachm of coal, gave fifty-five ounce meaſures of inflammable and fixed air.

The 1ſt	4	ounce meaſures was the air of the veſſel.	
2d	4	contained 80 fixed air and	20 inflam.
3d	26	95	5
4th	8	85	15
5th	10	30	70
6th	3	25	75
	55		

Half an ounce of the white oxyd of tin, and one drachm of coal, gave ſeventy-four ounce meaſures of inflammable and fixed air.

The 1ſt	4	ounce meaſures was the air of the veſſel.	
2d	12	contained 50 fixed air and	50 inflam.
3d	10	45	55
4th	26	20	80
5th	22	15	85
	74		

Half an ounce of the white oxyd of biſmuth, precipitated from a ſolution of biſmuth in the nitric acid by water, and one drachm of coal, gave thirty-eight ounce meaſures of inflammable and fixed air.

The 1ſt	4	ounce meaſures was the air of the veſſel.	
2d	10	contained 30 fixed air and	70 inflam.
3d	9	15	85
4th	10	8	92
5th	5	5	95
	38		

All theſe calces, except biſmuth and zinc, afford more fixed air than can be procured from coal and water, which ſeems to be a proof that they contain oxygen. Water appears to be a principal agent in producing part of the inflammable and fixed air, for theſe airs are procured in proportion to the quantity of this fluid in the metallic calces. That water is one of the agents, may be inferred from the difference in the quantity of the gaſes yielded by the ſame weight of metallic calx and coal. If oxygen was the ſole agent, no inflammable air could be obtained, and the fixed air would be greateſt from thoſe oxyds which contain the moſt oxygen, which is not the truth. Thus half an ounce of red lead, which is ſaid to con-

tain twelve per cent. oxygen, with one drachm of coal, gave but twenty-ſix ounce meaſures of inflammable and fixed air; while the ſame quantity of coal and white lead, which is ſaid to contain five per cent. oxygen, yielded fifty-three ounce meaſures of inflammable and fixed air, and the proportion of this laſt gas is three times as great as from the red lead. The oxyd of biſmuth too, which is ſaid to contain ſixteen per cent. oxygen, gave no more fixed air than could be obtained from coal and water.

The flowers of zinc, yielding no fixed air when ſubjected to heat with coal, or the fixed air which is ſometimes obtained being in proportion to the water which is united to the calx, is agreeable to the theory of Dr. Prieſtley, and cannot be accounted for by the new doctrine; and if the Doctor can prove, that as much fixed air can be procured from coal and water as from this ſubſtance and the metallic calces, a part of the antiphlogiſtic ſyſtem will be completely overturned: but to effect this, it will be neceſſary to ſhow, that from fifty to one hundred parts of fixed air may be made from coal and water, which has not yet been done.

SECONDLY. *Of a Solution of Copper in Volatile Alkali.*

Dr. Prieſtley ſays, a ſolution of copper in volatile alkali gave phlogiſticated air, and it will not be very eaſy to ſay where this azote exiſted before the proceſs.*

Copper was precipitated from a ſolution of blue vitriol by cauſtic pot-aſh, waſhed well with diſtilled water, and ammoniac was added to it: heat being applied, a part of it was diſſolved. The muriatic acid was then boiled upon it fifteen minutes, and no air except that of the veſſel was obtained.

One ounce meaſure of marine acid was alſo added to two ounce meaſures of a ſaturated ſolution of cuprum ammoniacum, and boiled with it one quarter of an hour, when nothing but the air of the veſſel was procured.

If Dr. Prieſtley obtained phlogiſticated air, it muſt have come from the volatile alkali, as it is one of its component parts.

* Medical Repoſitory, vol. ii. p. 386, firſt edition.

ARTICLE IV.

OBSERVATIONS *on the* YELLOW FEVER. *By Dr.* FELIX PASCALIS, *of Philadelphia.*

[Continued from page 17, and concluded.]

Section II.

I NOW come, Gentlemen, to the controverſy on ſalivation as a mode of treatment for the yellow fever.

Ptyaliſm, ſalivation, inflammation and ſuppuration of the parotid glands, with all the like, conſtitute various degrees of the ſame affection, which has often afforded to practitioners many phyſiological diſcuſſions, both for criſes and cure of diſeaſes: yet it never was well aſcertained what is the real proximate cauſe of that affection. It has appeared, indeed, that remedies as well as diſeaſes might excite a ſalivation. Its ſeat may be in all, or in many glands of the mouth, of the fauces, and in the ſalivary glands. Theſe are always affected, and ſometimes alſo the ſmall labial, and the glandulous body of the palate, with the ſalival miliary. All theſe glands becoming ſwelled, the membranes which cover them are inflamed, and frequently ulcerated. The progreſs of inflammation, in ſome of thoſe parts, is ſometimes ſo great as to check entirely the power of deglutition, and to cauſe the tongue to protrude and hang out of the mouth. The gums likewiſe participate of the affection: they are ſoon ulcerated and corroded, to the detriment of the teeth, which are frequently ſhaken out of their ſockets, or irreparably decayed. It is obvious that ſo great ravages, extended over various organs, and many delicate parts, make it very difficult to ſtop their progreſs by effectual remedies. Hence it happens that fatal effects often attend a ſalivation—ſuch as a mutilating gangrene of ſome part of the face, or of the internal membranes of the mouth, and even of the muſcles of the lower jaw: likewiſe a ſudden revulſion of the inflammation and fluids may take place from theſe glands into other viſcera, and cauſe a certain death.

In the various periods of this aſtoniſhing affection, whether produced by diſeaſe or promoted by medicines, we obſerve the ſame effects as in any local inflammation. There is a conſiderable influx of fluids, and a great heat; a reſolution afterwards is to be expected (unleſs induration or mortification

terminates the hiſtory of the caſe), which is ſerous or purulent. The firſt is frequently attended with hæmorrhage, and, conſequently, is very dangerous. The latter is deſirable; but it is remarkably offenſive, and ſeldom terminates without ſome conſiderable eroſions.—Theſe may utterly deſtroy the motions of the lower jaw.

It appears that the confluent ſmall-pox and malignant fevers are often attended with ſalivation. I have ſeen it myſelf in a caſe of tuberculous pulmonic conſumption.

Of all the medicines, mercury is the moſt powerful to promote ſalivation. It has been alſo aſcertained, that nitric acid, and other chemical preparations and acid ſubſtances, may excite a ptyaliſm with ſlight inflammation. It is obvious that theſe ſubſtances operate as ſtimulants immediately, applied to certain parts; but mercury excites ſalivation by being applied to any external part, or taken internally. Phyſiologiſts formerly thought, that a metaſtaſis of *morbific matter* was produced by mercury applied to the ſalivary glands, from which it was evacuated. This error was ſoon rejected, when it was found that, without any pre-exiſting diſeaſe or morbific matter, ſalivation might be completely effected; or that, after profuſe ſalivation, diſeaſe and morbific matter could exiſt the ſame. None of the modern theoriſts have been, I believe, very ſucceſsful in explaining the proximate cauſes of ſalivation. Whatever they think, it is now uſeleſs to repeat: I only obſerve, that, with oppoſite ſyſtems, many phyſicians have, in America, introduced the ſalivating method in almoſt every acute diſeaſe, and chiefly in the yellow fever. There muſt be ſome general reaſons why the ſame practical means are reſorted to, although with different theories; and of theſe motives I will hereafter mention thoſe which appear to be the principal and moſt plauſible.

All the arguments which have been held out by practitioners of different deſcriptions, in favour of the ſalivating method in the yellow fever, may be reduced, I believe, to three principal ones, reſorting to all kinds of theories. The firſt is, that it evacuates the poiſonous and ſpecific contagion of the yellow fever: the ſecond, that it overcomes the excitement, or deſtroys the *morbid action* of the blood-veſſels, produced by the preſence of the contagion: and the laſt, that it is juſtified by ſucceſs and obſervation in practice. We will now controvert theſe arguments, one after the other.

I do not ſuppoſe that there is any *ſpecific contagion* in the yellow fever to be evacuated by any extraordinary means;

yet, admitting this to be the caſe, I ſay that this contagious or ſpecific poiſon cannot be more eaſily ſubdued by ſalivation than that of the lues venerea; and it has been minutely aſcertained that the latter is not at all removed by the means of ſalivation. The opinion of the famous John Hunter on this ſubject is known, and I might mention alſo many celebrated French phyſiologiſts. Theſe have carried ſo far their opinion of the inefficacy of ſalivation, that they have highly recommended the mercurial treatment, called *par extinction*; in which the leaſt affection of the mouth is ſucceſsfully counteracted and avoided. Indeed, it is a doctrine generally allowed by all French practitioners, that the ſyphilis equally yields to mercury with or without ſalivation. In it they ſee nothing but a peculiar operation of the ſpecific on the ſyſtem, to which we ſhould not attend but to avoid its ravages. Fabre and Petit have ſatisfactorily proved, that if any evacuation is promoted by mercury, it muſt be either inſenſibly through the ſkin, or by the bowels. We adhere to the fact, that a regular mercurial treatment cauſes, before twenty days, a diſcharge from the bowels, which ſeems critical, and is generally attended with an effectual cure: and theſe medical conjectures ſtill more invalidate the utility of a ſalivating method. The ſame effects are obſerved in other diſeaſes of ſpecific contagion, and of obſtructed viſcera. We exhibit mercury with ſucceſs, if ſalivation has not taken place. This affection, therefore, cannot be called an effectual means for evacuating the poiſonous miaſmata that excite the yellow fever.

As for ſubduing the excitement or the morbid action of the blood-veſſels by ſalivation, this effect, if admitted, would prove the truth of what I have already ſtated of the loſs of animal irritability, cauſed by the preſence of deleterious gaſes: for the exceſſive increaſe of *excitement* ſuppoſes at leaſt a proportionate exhauſtion of *excitability* or irritability. Is it not a deluſion to think that a greater excitement pervades the ſyſtem, becauſe we ſee an exceſs of agitation in it, which has taken the place of the calm functions of a healthy ſtate? No, no—a perturbation of animal functions cannot be an increaſe of excitement. In the very meaning of our modern theories, excitement does not imply any thing inimical to the action of life, but what is favourable to it.

I know that they diſtinguiſh another ſort of excitement, which they call a *morbid action*, produced in blood-veſſels by hurtful powers. Good God! what is then that morbid action? Could it not be defined by ſome intelligible and phyſiological

words, in order that we could underſtand how it can be ſubdued by another hurtful power and morbid action in the mouth and ſalivary glands? Notwithſtanding we ſee but one natural effect in ſalivation, which is a new excitement added to excitement or morbid action: ſuch would be that of inflaming, by friction, the ſkin of a man intoxicated or highly *excited* by ardent ſpirits: therefore ſalivation has no excitement to ſubdue; or, if there is any, it will and muſt increaſe it, and give to it a ſtill greater character of morbid action.

But our theoriſts ſay, that as ſoon as ſalivation begins to operate, a favourable change of the malignant ſymptoms takes place. Indeed, the pulſe expands itſelf, and becomes more natural; the ſecretions recover their regularity; heat is equally diffuſed through the body; the *madar* of the ſkin promiſes a relief, and marks a happy and certain cure: to conclude, ſuch reſult is a proof that all the morbid action or excitement of blood-veſſels is abſtracted or ſubdued by ſalivation. We have ſeen ſome of thoſe effects, and we will examine hereafter with what reſult they can be compared to contrary effects. But this alteration of malignant ſymptoms, when it is effectually beneficial, does not more ſatisfactorily prove the abſtraction of excitement or morbid action, than it would prove, with other theoriſts, the tranſport or metaſtaſis, in the ſalivary glands, of the *morbific matter*—that is to ſay, of the ſpecific contagion or poiſon of the fever received into the blood. Even this latter ſuppoſition would be more rational, becauſe it means an immediate operation on the cauſe of the diſeaſe, whilſt the other relates only to its effect. It reſts with us, therefore, to point out what is to be thought of that alteration of ſymptoms produced by ſalivation, and how deluſive it is in many inſtances.

If I would contend further for the truth of my opinions reſpecting the loſs of animal irritability, which conſtitutes the yellow fever, and of the neceſſity of exciting it by artificial and ſtimulating means, I could, with as much conſiſtency as poſſible, exhibit the operation of ſalivation concurring effectually to that purpoſe, whether it is in itſelf a continued ſtimulating operation, or a peculiar operation of mercurial medicines, which indicates that the remedy has at laſt reſtored to the ſyſtem as much irritability as it formerly poſſeſſed. But in this controverſy let us renounce every kind of ſyſtematic theory, and reaſon only on facts univerſally obſerved.—Whatever may be the cauſe of fevers, and of malignant fevers too, there are none that cannot be cured by a local affection, or

by an evacuation. This is the origin of the doctrine of the criſes, which, ſince the days of Hippocrates, has been held out as the great ſubject of inquiries, for the ſafety of prognoſtics, and of clinical precepts. The ſcience of medicine has divided, as it ſeems, with nature, the taſk, or the *grand-oeuvre*, of producing thoſe curative *criſes* of local affections, and of evacuations.—The moſt ancient practice among the Chineſe and Japaneſe, was that of puncturing and burning the ſkin.* They ſtill adhere to it.—The Arabians and Egyptians are known to be the inventors of the *Moxa*. It is known, alſo, how much the celebrated Pouteau, of France, prevailed, in his time, to bring into credit the application of fire. The Dutch and the Germans ſtill continue the practice of cupping and ſcarifying. We are more improved in modern reſearches. We cannot do without cantharides, purges and emetics, becauſe local affections, by fire or by cauſtics, and evacuations, are neceſſary. It would involve me in too long a diſcuſſion, if I ſhould prove, beſides, what kind of local affections and ſpontaneous evacuations belong to every kind of fever; but it is a fact that they have their criſes, either by eruption, or local affection, or evacuation; and if one is unſucceſsful the patient muſt die.†

Among thoſe criſes, ſpontaneous ſalivation has been obſerved in ſome acute diſeaſes, in the confluent ſmall-pox, and in malignant fevers. It is true, that if conducted with regularity, it is the means of cure. Thoſe ſpontaneous ſalivations in malignant fevers, particularly in the yellow fever, have been obſerved and mentioned by eminent phyſicians: they have been accurately deſcribed by two French authors.‡ But we repeat it, that in order to prove beneficial, a ſalivation muſt be as ſpontaneous and laudable as an exanthematous eruption, or an abſceſs. Nature alone muſt direct its relieving operations. If theſe are artificially promoted, they become perilous or abortive in proportion as they are conſiderable, and attended with too many circumſtances of inflammation, &c. As for the apparent number of thoſe patients who ſeem efficaciouſly and eaſily relieved of their ſymptoms by a moderate ſalivation,

* Very intereſting and curious eſſays on theſe operations, as uſed among many ancient and oriental nations, may be found in the French Encyclopædia methodized, tom. i. art. Medicine, words *acupuncture* and *aduſtion*.

† Si cui ex febre ardente venit parotis quæ purulenta non fiat, haud facilè ſuperſtes evadit. Hipp. Coac. 138.

‡ Vide Maladies de St. Domingue, ſur la Fievre de Siam, by Pouppé Deſportes; and Maladies des Negres, ſur la Fievre de Siam, by Dazille.

it may be proved, that the ſlightneſs of their caſe accounts for that ſucceſs, which ſimpler remedies would have effected: for there are patients in whom no degree of ſalivary affection can be produced, and they invariably die. Again, there are patients who, with a profuſe ſalivation, do not curtail even the uſual periods of the diſeaſe, and a great number of thoſe will die. The proportion of victims is as great among ſalivating phyſicians as among thoſe who do not preſcribe it. During four ſeaſons of the prevalence of the epidemic, when I carefully avoided an artificial ſalivation, although I adminiſtered mercury as a diſcutient, and as a draſtic medicine, I never perceived that I was leſs fortunate with my method than other phyſicians. If I muſt ſay it, the only difference which ſtruck my obſervation was, that many more patients, on one ſide, recovered within a ſhort period of time, and without tedious convaleſcence, compared to thoſe who, by the long and painful effects of a ſalivation, are longer kept under the action of diſeaſe, and ſometimes linger under more ſerious effects.

Finally, if the method of ſalivation would be productive of a ſmall advantage, its application ſhould ſtill be made liable to a great many exceptions of caſes in which the remedy becomes worſe than the diſeaſe. The irritability or ſenſibility of women and children renders them unfit for its violent operation. In ſuch ſubjects, it ſoon overcomes their natural powers, produces convulſive effects, aggravates all ſymptoms, and moſt ſurely removes the opportunity of adminiſtering more ſafe remedies. People advanced in years, whoſe teeth and gums are in a periſhing condition, cannot bear ſuch morbid affection without a great addition to their ſufferings and dangers. Scorbutic patients, and thoſe previouſly affected with indurations of ſalivary glands, or any other ſcrophulous affections, ſhould, without exception, be forbidden a ſalivating courſe; becauſe, in ſuch ſubjects, pre-exiſting diſeaſes would be aggravated to an alarming degree. The proportion of all thoſe different deſcriptions of people leaves but a ſmall number to whom this remedy may prove perfectly indifferent; and, with reſpect to theſe, we are led to doubt, whether mercury, adminiſtered internally, as a diſcutient or as a purging remedy, would not more readily contribute to their recovery; and, if ſo, let the method of ſalivation be expunged from our liſt of remedies for the yellow fever. It will be objected, that even without promoting the affection of the mouth by ſufficient doſes of calomel, the leaſt quantity of mercury, adminiſtered as a purge, may, in many inſtances,

excite a complete ſalivation. I anſwer, that, provided a ſmall mercurial mixture be made with nitre and jalap, and ſometimes with opium and certain oxyds of antimony—provided alſo thoſe doſes be not repeated unleſs the preceding has operated through the alimentary canal, ſalivation will ſeldom, if ever, take place. If it does, when the promoting cauſe is very inordinate, it will then be judged as a ſpontaneous criſis, and, for that reaſon, much more to be depended on as uſeful and beneficial.

Section III.

Another important queſtion has greatly divided the American faculty, while it was conſidered, by all parties, as cloſely connected with the ſafety of the inhabitants and neighbours of the places where the epidemic has been known to rage. I muſt confeſs, however, that I never heard nor read that ſuch queſtion had been treated in its true meaning, viz. If the yellow fever is imported, will it, or not, pervade a whole population, and a whole country, by its contagious power?

It is obvious that the believers in the domeſtic origin are not required to anſwer this queſtion; but the others are, and they have ſaid nothing yet that is perfectly conſiſtent with facts and doctrine. It has, however, been ſaid, that the diſeaſe was contagious, and that, if aided by local circumſtances of climate and exhalations, it muſt pervade inhabited places. This mode of reaſoning has been *officially* tranſmitted to legiſlative bodies! and while it has been made the leading principle of legiſlative meaſures, the believers of the domeſtic origin have ſeen, with pleaſure, that the occurrence of ſome domeſtic caſes was acknowledged; and they have remained ſomewhat indifferent as to the queſtion of the contagious power of the yellow fever. Dr. Ruſh has lately pronounced, *that it is rarely and feebly ſo.* Others deny that, in any manner, it can be ſo. One thing, therefore, remains abſolutely undecided among the learned, which is, whether the yellow fever is or is not contagious: and as, in the affirmative of the queſtion, it is not underſtood that, by its contagious power, the epidemic could pervade a whole population; and, in the negative, no arguments are exhibited againſt ſtriking facts of contagion, we may fairly conclude that we do not underſtand each other, and that ſome other grounds of the theory of contagious ſickneſs muſt be found, to be applied to this ſubject, and to conciliate parties.

I confeſs, Gentlemen, that as I have been myſelf an active witneſs of the epidemic, during many years of its prevalence, I was long perplexed in my mind to conciliate facts with ſome conſiſtent theories—and in that I never was ſatisfied until lately. Without regard to any ſyſtem, or to conteſted doctrines, it has appeared to me, as well as to many practitioners, that a conſiderable number of the caſes could not be traced but to a *contagious power* of the fever itſelf. Such were the caſes of whole families, who ſeemed preſerved as long as they had no patient in their houſe, and who all periſhed or were ſick, without exception, as ſoon as they admitted among them ſomebody affected with the diſeaſe. This deplorable effect has been ſeen in the moſt wholeſome parts of the town, and at any period of the ſeaſon; ſo that, in many inſtances, when the diſeaſe ſeemed moſt univerſal, by care, precaution and deſertion, people were preſerved; while, in the country, they fell victims to their unreſerved intercourſe with patients, when the epidemic was faſt decreaſing in every part of the town. On the other ſide, to attentive obſervers a quite oppoſite reſult has been aſcertained, beyond any kind of doubt. Not one experiment has favoured even the probability of a ſpecific contagion being, in the yellow fever, communicable by inoculation.

We do not know any ſymptom, *ſui generis*, by which, in the yellow fever, as in any *poiſonous diſeaſe*, the abſorbed virus is thought to lay in certain parts, or be diffuſed in the whole ſyſtem.

Such are a few grounds, among many others, for contradictory doctrine: and to the honour of medical ſcience, I think it right to ſay, that, without a new theory, we do not know any thing of the contagious power of the yellow fever.

Behold, Gentlemen, and pleaſe to compare with ſo many oppoſite facts the views which I humbly propoſe to the American faculty on that ever intereſting ſubject, for the preſervation of their fellow-citizens, and they will be reconciled—they will all appear concluſive and neceſſary. Admit that the remote cauſe of our epidemic is a floating gas in the atmoſphere, diſengaged from putrefiable and fermented ſubſtances—admit that theſe gaſes are a *negative contagion*; that is to ſay, operating by depriving animated matter of its *irritability*, and it will be evident that they affect bodies by throwing them into a putrefiable ſtate; and the yellow fever appears to be a diſeaſe in which putrefaction begins before death. Thence we may de-

rive a ſtill more neceſſary reſult, that of ammoniacal* and deleterious effluvia ariſing from ſick perſons, even when they are not in a ſtate of perſpiration, which, in its ſedative operation on thoſe who receive it, will be expanded and envenomed by heat, as the exhalations ariſing from the ground: and this incontrovertible law will explain the phenomena of contagion, which we are acquainted with, but which we cannot comprehend and define in any other manner than that of *negative contagion*; yet ſuch noxious power will be underſtood, and limited within certain bounds: thus it will not be ſufficient to pervade a whole population, and will be ſtill leſs fatal than that of the general impurity of the atmoſphere, which we know may be varied by ſo many rapid cauſes.—Finally, let us admit theſe facts, and we will be ſatisfied that thoſe who attend the ſick, in open, airy and clean places, like our hoſpitals, will ſeldom be affected with the prevailing epidemic; but in confined and unclean places the danger will be greater, as we have already experienced: therefore there cannot be any *ſpecific* poiſon, or *contagious* virus, affixed to perſons, nor to their clothes, nor ſwimming in their fluids, nor impregnating their fleſh. The ſick mother may, to the laſt, ſuckle her innocent babe, who ſhall not have been infected with the fatal gas, and all attempts in diſſecting, mangling, and inoculating, will prove, that in the yellow fever there is no *ſpecific contagion.*

Thoſe who attend patients in confined places are in danger of being affected with the ſame malignant fever, by the hepatic or deleterious gas ariſing from their body. This negative contagion may be effectually checked by all thoſe means which have a tendency to purify or renew the air of the ſick chambers. For this purpoſe, it may be recommended to keep conſtantly a burning fire on the hearth; to place a certain quantity of ice (which muſt be inceſſantly renewed when melted), in any veſſel, near to the beds of the diſeaſed; to have a few vaſes or pots with living vegetables; and, laſtly, to keep,

* This gas muſt be more dangerous than thoſe formed and diſengaged from other vegetable or animal matter, which are, in general, more ſimple in their principles, and very ſoon diluted or neutralized in the atmoſphere. But the gas ariſing from living putrefaction, and ſaturated with caloric, when emitted in bed-rooms, is a compound of hydrogen, carbon, and phoſphorus, and, with them, muſt form every ſpecies of deleterious *azurets.*

in proper veſſels, metallic oxyds of mercury, lead, or manganeſe, in ſolution with concentrated ſulphuric acid, which, as is known, will produce and diffuſe a new oxygen.

I have the honour to be, reſpectfully,
Gentlemen,
Your moſt obedient, humble ſervant,
FELIX PASCALIS.

ARTICLE V.

A SKETCH *of the* HISTORY *of the* DISEASES *of the* STATE *of* DELAWARE: *Communicated in a Letter from Dr.* JOHN VAUGHAN, *of Wilmington, to Dr.* MILLER.

No. III.

DEAR SIR, *Wilmington, June* 10, 1800.

HAVING concluded the liſt of autumnal fevers in No. II. we muſt enumerate their ſucceſſors in the morbid catalogue.

The month of November is uſually healthy, except it be from the remains of febrile diſeaſes—as *ædematous* and *anaſarcous* ſwellings, anorexia and muſcular debility, with occaſional feveriſh attacks, uſually denominated *inward fevers.* Thoſe affections being dependant on the ſame cauſe, are eaſily removed by the ſame proceſs, *i. e.* a reſtorative regimen, exerciſe, tonics, friction, &c. But if anaſarca be attended with ſchirroſities of the ſpleen and liver, they are frequently troubleſome and fatal. If they are not neglected, a courſe of mercurial gummous alteratives, with friction, is generally ſucceſsful; but if *aſcites* be added to the liſt, and the patient's ſtrength is much exhauſted, a lingering death pretty certainly enſues.

If there be much wet weather attending the tranſition from heat to cold, as is not unfrequently the caſe, a ſeries of troubleſome affections is induced—as catarrhs, mucilaginous diarrhœas, eryſipelas, &c. the particular treatment of which cannot be detailed in the limits of an eſſay of this kind. Flannel ſhirts are the moſt effectual preſervatives, and the common reſort of valetudinarians.

December and January are alſo healthy, if the weather be dry and uniform; but if the weather prove variable, accompanied with rain or ſnow, without ſevere froſt, pneumonic diſeaſes and catarrhs become epidemic. February, however, is the *proverbially morbid* month of winter. The miaſmata of autumn are effectually deſtroyed by froſt—a reſpite has been obtained from the attacks of *ague*, and a ſufficient quantity of oxygen inhaled to give riſe to a new order of diſeaſes, whoſe remote cauſe is ſudden tranſitions in the temperature of the weather. Thus, unfortunately for the late ſubjects of the *ague*, &c. they are not ſufficiently *oxygenated* to endure expoſure to the viciſſitudes of the elements. The impreſſion of cold produces irregular excitement, or convulſive arterial action—the famous *vis medicatrix naturæ* is deſtroyed by a ſingle north-weſtern blaſt, or a wetting of the feet, and the trembling victim hurried to his irkſome bed: or the mode of attack may be varied by incidental circumſtances. The diſeaſe generally commences with the uſual ſymptoms of febrile diſeaſes. If the attacks be ſudden and ſevere, the violence of the caſe is announced; but, in moſt caſes, irregular feveriſh heats and chills, with the ordinary ſymptoms of catarrh, form an inauſpicious prelude of three or four days duration. The affection of the breaſt now becomes inſupportable—cough inceſſant, but attended with an expectoration of frothy mucus—great oppreſſion at the præcordia, with a ſenſe of ſoreneſs, but ſeldom acute pain. On the ſecond or third day the cough becomes more violent—expectoration ſuppreſſed—the head attacked with pain and confuſion—the tongue furred and parched—the ſkin dry and hot—the pulſe frequent and ſoft. On the fourth or fifth day thoſe ſymptoms are aggravated—the patient becomes iraſcible, with ſudden ſtartings from apparent ſleep, and great anxiety; and, as his ſtrength is exhauſted, the pulſe becomes more frequent and irregular—the delirium is a typhomania—the eyes aſſume a glaſſy appearance—the extremities gradually loſe their heat—the reſpiration reſembles uninterrupted ſighing—and, from twenty-four to forty-eight hours, cloſe the diſtreſſing ſcene. A jaundice frequently pervades the body, ſhortly before, or immediately after death, and often petechiæ. The bowels are either conſtipated or affected with diarrhœa.

In the more violent or active forms of this diſeaſe the ſymptoms are varied, approaching towards ordinary pneumonia—the pulſe is full, but not tenſe—the expectoration copious, and ſtreaked with blood on the third or fourth day—pain of the

breaſt more acute, and the oppreſſion leſs: yet, in many caſes, the ſcene is reverſed, and the diſeaſe terminates in twenty-four or forty-eight hours.

The general character of this diſeaſe is a *peripneumonia notha*, which, doubtleſs, is juſt in a majority of caſes; yet I am rather apprehenſive that the denomination is deluſive: for, in numerous inſtances, the general ſtate of the ſyſtem is diſregarded—the local affection is the only object of concern, as the fever is often ſo irregular as to eſcape attention. It is not an uncommon thing to be informed by the patient, that his only complaint is an uneaſineſs in the breaſt, or an affection of the head, when the extremities are becoming cold, the tongue fiſſured, ſtupor gliding on, and Protean Death aſſuming his ſeat. I, however, hope not to be miſunderſtood. To ſay that thoſe local affections do not merit regard, would be an abſurdity indeed: to ſay that a congeſtion of the *lungs* or *brain* is an incident of no importance, would be equally novel and ridiculous: but to diſregard the unfortunate diatheſis of my patient, would be to enſure him a ſpeedy death. I have witneſſed a number of caſes where the local affections only were conſidered—the diſeaſe termed pleuriſy, or phrenitis, and depletion commenced *ſecundum artem*, in which none of the patients ſurvived forty-eight hours: and I ſhould, probably, have fallen into the ſame error, had not the ſtate of the pulſe—the frequent ſighing in reſpiration—the tendency of the fever to aſſume a tertian type—the early appearance of the Hippocratic countenance, ſo called, with other ſymptoms more eaſily obſerved than deſcribed, convinced me that it belonged to the genus of autumnal fevers, varied by the caſualties of the atmoſphere. It may, perhaps, be ſaid, that this poſition is rather too far ſtrained; that froſt deſtroys marſh miaſmata as a tropical plant. Granted; when the ponds are covered with ice exhalation is overpowered. But if theſe effects ſuffer a common fate with their cauſes, why do intermittents exiſt, or occur, in winter? and winter quartans are the moſt obſtinate of the whole tribe. Why do valetudinarians ſuffer relapſes in the froſts of January and February? And why are perſons, on a removal from a marſhy to a high country, attacked with the endemial fevers of the *fens*, if the remote cauſe be not dormant in the ſyſtem, and excited into action by a concurrence of prediſpoſing cauſes? Theſe facts, I preſume, ſubſtantiate the poſition, that our winter diſeaſes are but varied forms of the *autumnal* fever. Hence the propriety of the following denominations, agreeably to the ſyſtem of

Dr. Ruſh, to whoſe pathological merits every impartial obſerver muſt bear teſtimony; *i. e.* if the head be the ſeat of topical action, it is a *phrenitic* ſtate of fever—if the breaſt, it is a *pneumonic* ſtate of fever—if the bowels, it is an *inteſtinal* ſtate of fever, &c. &c.

On this arrangement I proceed to the method of treatment, which every reader muſt acknowledge to be ambiguous: and as it is difficult to evince the peculiarities of the diſeaſe in a general deſcription, I ſhall narrate the following caſes, to ſerve as a ſpecimen of *its* nature.

P. C. a female, aged twenty-five years, of a florid complexion, and habituated to laborious exerciſe, was attacked, on the evening of the 3d of January, 1796, with a chilly fit, ſucceeded by fever, cough, oppreſſion at the præcordia, &c. I was called at 10 o'clock A. M. of the 4th, when her pulſe was imperceptible, reſpiration converted into ſighing, the ſurface cold, and covered with purple ſpots. In a few minutes a great quantity of granulated blood burſt from her mouth, noſe and ears. I ſtood a paſſive ſpectator of her unceremonious fate!

S. H. an active robuſt man, aged 26 years, indiſpoſed by a ſlight catarrh, was attacked with a chill at a fox-chaſe, on the 10th of March, 1797. During the chill he drank freely of cool water, and vomited inceſſantly. Fever ſucceeded; and about an hour from its commencement, I ſaw him. He had a frequent dry cough, oppreſſion and pain in the breaſt, pain in the head, pulſe full and rather tenſe; and, in fact, the ſymptomata of pneumonia complete. I opened a vein, and drew ℥ xij. of blood; gave an effervescent mixture to check the vomiting, and afterwards an antimonial febrifuge to promote perſpiration. At nine o'clock P. M. he was much relieved, and inclined to ſleep.

On the 11th, at 7 o'clock A. M. I viſited him; and, to my great aſtoniſhment, found him quite exhauſted. His pulſe feeble—extremities cold, and of a leaden hue—his lips and tongue covered with a dark ſcurf, and fiſſured—his eyes of a glaſſy appearance, and the pupils ſomewhat dilated—with typhomania.

Bliſters were applied, and cardiacs given; but to no purpoſe. He died comatoſe at 4 o'clock P. M. *eodem die.*

"A ſudden ruſh from life's meridian joys!"

A variety of ſimilar caſes occurred this ſpring; but moſt of them were confined to the centre of the Welſh-Tract Swamps,

a few miles below me. A conſiderable portion of them were the *phrenitic* ſtate of fever, and uniformly fatal. Whereas, the mortality of the ordinary grade of the *pneumonic* ſtate of fever, on my regiſter, is about one in ten. And it muſt be obſerved, that the ſubjects of this diſeaſe are generally the debilitated, perſons advanced in years, and the diſciples of antifogmatiſm.*

A diſeaſe thus varied, ſubject to ſuch ſudden tranſitions, and anomalous in its nature, is not leſs difficult to cure, than to detail a ſyſtem of its treatment to others. It is much like comparing the complaints of a patient with the diagnoſis of noſology, and ſelecting a preſcription from ſome good old book, whoſe author is grown hoary in forming recipes, or fled from earth an age before. It is ſo much varied by the habit of the patient, ſtate of the weather, and other incidental circumſtances, that it is almoſt impoſſible to deſcribe a general method of treatment. However, I ſhall give ſuch detail as the nature of the circumſtances will admit.

If there were any fulneſs of the pulſe, with conſiderable pain of the breaſt or ſide, and breathing difficult, without ſighing, I bled more or leſs, *ex rei neceſſitate*, evacuated the bowels, and gave the uſual antimonial diaphoretics, with calomel. If the pain of the breaſt were not relieved by the blood-letting, bliſters were applied to the cheſt and ankles, and to the neck, if the head were diſturbed. Cathartics were ſeldom repeated, and coſtiveneſs was obviated by injections. If there were any affections of the liver, a bliſter was applied to the hypochondrium. If typhous ſymptoms came on, and the expectoration partial, the bliſters were renewed—opium, with camphor, or a ſolution of volatile alkali, was given—or the neutral mixture, with a decoction of ſnake-root.

Demulcents were given to allay the cough—as linſeed-tea, liquorice, &c.

A favourable criſis moſtly happened on the third, fifth, or ſeventh day—announced by a copious expectoration, ſtreaked with blood—a diaphoreſis, and ſubſequent relief of all the urgent ſymptoms.

In ſome caſes, the inhalation of the vapour of linſeed-tea, or warm water with honey, contributed much towards relieving the cough.

In fatal terminations, the ſkin remains dry and parched—expectoration partial—bliſters to the extremities do not re-

* In ſome of the middle and ſouthern States drams are called antifogmatics.

ſtore their heat—the tongue is furred and fiſſured—and ſighing and coma cloſe the cataſtrophe!

It will be readily obſerved, that this fever belongs to the ſynochoid genus, and that its tranſition to typhus is often uncommonly rapid; and, in fact, it was ſometimes typhus in the commencement. The ſuddenneſs of the tranſitions made me extremely cautious in the uſe of the lancet, unleſs it was ſtrongly indicated by the acuteneſs of the ſymptoms. The mixed nature of this diſeaſe—the apparent inflammation of the brain or lungs—with ſubſultus tendinum, frequent pulſe, and great proſtration of ſtrength, have cauſed me many an anxious day's reflection on the uſe of the lancet: the patient apparently ſuffering from omiſſion, and general debility portending death, even without its uſe.

Dr. Moore, in his Medical Sketches, mentions there being *grey diſeaſes*, as well as *grey cauſes* in law; and we may, doubtleſs, annex *this genus* to his liſt.

This haſty ſketch will conclude my propoſed hiſtory of the diſeaſes of Delaware, in an annual form; and, I hope, the deſign will ſerve as an apology for the defects it may contain. The object of ſuch communications is a reciprocation of information reſpecting endemial diſeaſes—their forms—periods of occurrence—influence of the ſenſible qualities of the atmoſphere—and ſuch articles in the treatment as depend on the local nature of the diſeaſes.

Theſe have been my objects; and how far I have ſucceeded you and the public muſt decide; and, at leaſt, accept the will for the deed. If exceptions be made, my reſponſe is, "Come thou and do likewiſe!" With ſincere eſteem,

I am your friend and humble ſervant,

JOHN VAUGHAN.

ARTICLE VI.

EXPERIMENTS *on* HEATING MANGANESE *in* INFLAMMABLE AIR: *In a Letter from the Rev. Dr.* PRIESTLEY *to Dr.* MITCHILL.

DEAR SIR,

I DID not intend to trouble you, or the public, with any more obſervations on the *new theory of chemiſtry*, after my former communications on the ſubject; but having lately

made ſome experiments on heating manganeſe in inflammable air, which, though they authoriſe the ſame general concluſion with thoſe on heating red precipitate in that air, are conſiderably different from them, I think them deſerving of attention.

Manganeſe and the red precipitate of mercury are ſubſtances of a ſimilar nature, as they are both calces of metals, and both contain dephlogiſticated air; and, like all other calces of metals, they imbibe the inflammable air in which they are heated. But, whereas the dephlogiſticated air expelled from the precipitate, frequently at leaſt, remains mixed with the remainder of the inflammable air, which is ſometimes the cauſe of dangerous exploſions, this has never happened to me in conſequence of heating manganeſe in the ſame circumſtances; ſo that it may be operated upon without any apprehenſion.

Though, in theſe experiments, the inflammable air diſappears, and dephlogiſticated air is expelled from both the ſubſtances, no *water* is formed by means of either of them, as the new theory abſolutely requires, and which is pretended to be the caſe when finery cinder is heated in the ſame circumſtances. But, ſince the ſame quantity of inflammable air diſappears in all theſe caſes alike, the ſame quantity of water, if inflammable air be really a conſtituent part of it, ought to be produced. It is evident, therefore, that the water procured by means of finery cinder was actually contained in that ſubſtance, and only expelled by the heat, to make room for the phlogiſton, which then enters into it by the abſorption of the inflammable air; while the precipitate and the manganeſe, containing no water, imbibe the inflammable air without any appearance of water, except the little that is the baſis of the inflammable air, or that may be concealed in ſuch powdery ſubſtances as manganeſe and the precipitate are.

The principal difference, however, in the reſult of theſe two ſimilar proceſſes, is, that after the experiment with the manganeſe a quantity of *phlogiſticated air* appears to be generated, which is not the caſe with the precipitate. The particulars of the experiments were as follow:

Fifteen ounce meaſures of inflammable air were reduced to ſeven by heating in it twenty-four grains of manganeſe. In the reſiduum, which was not affected by nitrous air, there was no fixed air; but when a certain portion of it was fired together with a certain quantity of dephlogiſticated air, the diminution was only to 0.95; whereas, when the ſame quantity of the original inflammable air was exploded in the ſame circumſtances, the diminution was to 0.52. In another ſimi-

lar experiment, in which the inflammable air was confined by mercury, and in which ſeven ounce meaſures were reduced to one and a quarter, the diminution with the reſiduum was to 0.93, when that with the original inflammable air was to 0.33. The manganeſe I uſed in theſe experiments yielded dephogiſticated air only, without any mixture of fixed air.

I repeated this experiment with manganeſe, out of which air had been expelled by a ſtrong heat, and the reſult was the ſame as with the freſh manganeſe, except that much more heat was neceſſary to make it imbibe the air; whereas the manganeſe, from which the dephlogiſticated air has not been expelled, imbibes inflammable air very rapidly, and with a very moderate degree of heat. Twenty grains of it imbibed eight ounce meaſures of inflammable air, and then weighed nineteen grains. In both the kinds of manganeſe, the central part, on which the focus of the lens fell, was of a beautiful green colour; but the next day this had diſappeared, and the whole of the ſubſtance that had been heated was brown.

As, no doubt, dephlogiſticated air is expelled from the manganeſe in theſe experiments, and neither fixed air nor water is produced, the dephlogiſticated air ſeems to enter into the phlogiſticated air that is found in the reſiduum; and yet it appears, from ſeveral of my late experiments, an account of which I have ſent to the Philoſophical Society at Philadelphia, that phlogiſticated air is ſometimes formed without dephlogiſticated air, which is a difficulty that I am not able to ſolve.

In order to make a more exact compariſon between the experiments in which *manganeſe* and *finery cinder* were made to imbibe inflammable air, I repeated them both, in ſimilar veſſels, with equal quantities of inflammable air, confined by mercury; and I obſerved that, though ſome water appeared during the proceſs with the manganeſe, it wholly diſappeared the next morning; whereas the inſide of the veſſel in which the finery cinder had been revived was wholly covered with innumerable drops of water, contiguous to one another.

ARTICLE VII.

An ACCOUNT *of a* DISSECTION, *with* REMARKS *thereon: By* GEORGE LEE, *A. M. addreſſed to Profeſſor* MITCHILL.

JAMES TARNEY, a labourer, aged 65, was admitted into the Pennſylvania Hoſpital on the 2d of December, 1797, for a large tumour on the right ſide of the cheſt, which had made its appearance about fifteen months before. He could aſſign no cauſe for it, except that he had received, many years ago, a ſmall wound upon the upper bone of the ſternum.

The tumour being ſituated upon the right ſide of the thorax, extended from the ſecond to the ſeventh rib, and from the cartilages of the ribs to within a ſmall diſtance from their angles. The tumour was opened twice by Dr. Phyſick, and diſcharged, at both times, five pints of glare. He expectorated this glary matter, mixed with mucus, ſeveral days previous to his death; and could not lie in a horizontal poſition, during that time, on account of the matter riſing up into the trachea in ſuch quantity as to threaten ſtrangulation.—He finally died in a very eaſy manner, and at an unexpected time.—After death, the following were the appearances upon diſſection.

The integuments which covered the tumour being removed, we found the pectoralis major entire; and, upon making a crucial inciſion into it, the cavity of the tumour was brought into view, which diſcovered to us a beautiful net-work of cellular membrane, paſſing from one ſide to the other, with a ſmall number of blood-veſſels ramifying through it. Theſe cells were undoubtedly the cavities which contained the glare diſcharged by the operation before death. But the matter we found in them was of the conſiſtence of tallow, and of a white appearance. In the centre of the tumour a ſmall quantity of bloody puriform matter was found, and ſome of the originating ſlips of the ſerratus magnus anticus muſcle were conſiderably deranged. No ribs were found within the tumour, except at its circumference, where ſmall remains of them were to be ſeen, apparently in a half corroded ſtate. The ribs which ſuffered the greateſt obliteration were the third, fourth and fifth. The ſixth was partially deſtroyed.

The tumour was ſemi-ſpherical, reſting on the pleura,

which, in this place, inſtead of being convex externally, as it naturally is, was concave. No direct communication could be diſcovered between the cavity of the tumour and that of the thorax; for the pleura appeared entire and natural, except being a little indurated and puriform on its external ſurface. But that there had been a communication was evident from three circumſtances: 1ſt. He threw up from the lungs, before death, the ſame kind of matter which was diſcharged from the tumour by the puncture. 2d. The matter flowed more freely during ſudden reſpiration than in any other act of breathing: and, 3d. The matter contained in the tumour, after death, very much reſembled that found in the lungs, as will be mentioned preſently.

The lungs ſeemed to be compoſed, as it were, of an immenſe number of tubercles, generally ſpeaking, about three lines in diameter. They were white, or, perhaps more ſtrictly, grey; firmer than tallow, and a little puriform in the middle. The left lung appeared as tuberculous as the right, and they both adhered very firmly to the pleura in every place (except to the pericardium), as much to the diaphragm and mediaſtinum as to the ſides of the thorax. No trace from the wound he had received upon the ſternum could be found.

Upon opening the abdomen, the firſt thing which attracted notice was the ſtomach. This viſcus, which was very large, inſtead of being in its natural oblique ſituation, was found lying lengthwiſe on the left ſide, anterior to the ſpleen, and contiguous to the peritoneum, to which it adhered. The pylorus was curved up in conſequence of its attachment to the duodenum, which inteſtine was in ſome meaſure diſplaced; probably by the weight of the ſtomach, when full, dragging it downwards. I once heard of the ſtomach being ſituated nearly in the ſame manner, but I believe ſuch an appearance is ſeldom met with. A few remains only of the omentum were ſeen, which was to be expected from the emaciated ſtate of the ſubject. The liver was much harder than it is found in a natural ſtate, and appeared contracted. It adhered to the peritoneum. This is one, and the moſt common kind of liver, which perſons who are intemperate in the uſe of ardent ſpirit often poſſeſs. I have ſeen many inſtances of it; and, in ſuch caſes, the liver is often whitiſh, and of a granulated texture. I once diſſected an intemperate man, whoſe liver was granulated, or, to uſe a vulgar phraſe, *knotty*, and ſo much contracted and hard that it was with difficulty an inciſion could be made into it; and the noiſe the ſcalpel made in paſſing through it

much reſembled the cutting of dry ſoal-leather, the reſiſtance only not being ſo great.* The inteſtines were contracted, thickened, and tough—adhering very ſtrongly to each other, and to the peritoneum, on each ſide of the ſpine, to the diſtance of at leaſt four inches. The urinary bladder ſeemed tied in a preternatural manner to the pubes. This, as well as all the morbid adheſions we met with, were, no doubt, produced by inflammation. Some days after his death, I was informed, from good authority, that he had been very intemperate at different periods of life. No other diſeaſed appearances were found worthy of notice; and I will now make a few remarks upon the caſe, as it has been ſtated.

The diſappearance of ribs is a fact which conſtitutes the moſt intereſting part of the hiſtory of the foregoing caſe; and is one which, I think, does not admit of an eaſy explanation. We can eaſily conceive how nature removes fluid ſubſtances from different parts of the body; but how ſhe carries off the more ſolid materials of which we are compoſed, is a phenomenon which, whilſt it excites aſtoniſhment, and calls forth our admiration, keeps aloof every conjecture concerning the manner in which it is effected. We are all ready to believe that the abſorbents are capable, either by capillary attraction, or by a muſcular power, of taking up effuſed fluids; but it is a difficulty, on the other hand, under which phyſiologiſts labour, to conceive how they abſorb bone and other ſolids. It has been the prerogative of the ingenious, which right they have often exerciſed, to teach us that, before an abſorption of the ſolids takes place, they are made fluid. But this is only creating one difficulty in order to ſolve another: for it is well known we have no proof on which to ground the aſſertion, however anxious we may be to make uſe of it. I do not mean, however, that the fact is not as ſome men would have it. So diſtant is my opinion from this, that I am diſpoſed to admit ſomething of that nature does take place. But until this doctrine is better ſupported by the evidence of truth, drawn from experience, few men of accuracy will give their aſſent to it. Yet, however averſe we may be to reaſon on this intereſting ſubject, on account of its abſtruſeneſs and difficulty, the fact ſtill recurs to our view, and its preſence commands that attention which is due to its importance. You will permit me to call your attention here to a few intereſting facts, relative

* It is curious that in this caſe the ſecretion of bile appeared to be unimpaired.

to the preſent ſubject. The diſeaſe called mollities oſſium is owing to too great an abſorption of calcareous earth from the bones. In proof of this, we have an intereſting hiſtory, by Mr. Thomſon,* of a James Stevenſon, "whoſe urine, during the two firſt years of his affliction with this diſeaſe, generally depoſited a whitiſh ſediment, which, upon evaporation, became like mortar." In this caſe, it is probable, the earthy matter, after having been abſorbed from the bones by the lymphatics, was carried out of the ſyſtem, by the action of the kidneys, in the ſecretion of urine. Mr. John Bell† believes that mollities oſſium is produced by an abſorption of bone, and that, during the continuance of the diſeaſe, earthy matter is diſcharged *per urethram*. Meſſrs. Cruikſhank‡ and Sheldon§ entertain the ſame idea of this diſeaſe. When a bone exfoliates, the decayed part is thrown off, by an abſorption of a portion of the living, in contact with the dead. The alveolar proceſſes of the jaws diſappear in old people when the teeth fall out. Mr. Cruikſhank‖ mentions a caſe where the ſides of the tibia were reduced to the thickneſs of a wafer; and I once attended a woman who fractured the bones of both legs, juſt above the ankles, by only ſtepping obliquely upon the edges of bricks. Theſe caſes, no doubt, aroſe from the abſorption of the bones rendering them very weak. All the hollow cylindrical bones, and the cranium, during their growth, undergo an abſorption as well as a renewal of their ſubſtance. Whilſt the lymphatics are removing matter from their internal, the arteries are depoſiting new particles of bone upon their external ſurfaces. And, in this particular, the growth of a bone reſembles that of wood, only that there is no abſorption in the latter.

If there was no abſorption during the growth of theſe bones, how could their cavities enlarge? and if new layers of bone were not formed on the external ſurfaces of them, how could they be extended? It is not only true that this is the courſe of nature during the growth of bones, but it ſeems very probable that it is continued during life. That this reciprocal action, between the arteries and abſorbents, does exiſt in every part of a bone during life, is rendered ſomewhat evident by one fact: If an animal be fed upon madder, the whole ſub-

* Medical Obſervations and Inquiries, vol. v. No. 23.
† Anatomy of the Muſcles, Bones, and Joints.
‡ Anatomy of the Abſorbing Veſſels, p. 111.
§ Hiſtory of the Abſorbent Syſtem, p. 31.
‖ Anat. Abſorb. Veſſels, p. 104.

ſtances of its bones will be coloured red by it, which tinge, in proceſs of time, will vaniſh. In old people, the bones have loſt a conſiderable ſum of their former weight;* which proves that the abſorbents are more active at this period of life than the arteries. The various changes which the under-jaw ſuffers during life, depend entirely upon the action of its lymphatics. A venereal node, which is owing to a ſwelling or increaſe of bone, will be abſorbed by the lymphatics when ſtimulated by mercury. The ſurpriſing configuration of the bones of different nations has been derived from their peculiar cuſtoms. Thus, the uſe of a turban, according to Veſalius, produces in the Turk a conical ſkull; and the early employment of the chin-ſtay gives a flat head to the Engliſh. I ſome time ſince ſaw an Indian ſkull, found on the banks of the Miſſiſſippi, the top of which was raiſed in the form of a ſpine, running from before backwards, ſuppoſed to be produced by the application of two boards, during infancy, to each ſide of the head. Beſides theſe facts, it is to be recollected, that all the bones of the body yield to, and are impreſſed by, the contraction of a muſcle, the beating of an artery, &c.

Beſides theſe facts relative to the abſorption of bone, I could, were it neceſſary, multiply them, as well as adduce many in proof of the abſorption of the ſoft ſolids, which appears to me equally important and difficult of explanation with the removal of bone by the lymphatics. If the abſorbing veſſels deſtroy glands and other ſoft ſolids, which they certainly do, there is little doubt in my mind, that they are completely adequate to the abſorption of bone: for if it is neceſſary that bone ſhould be previouſly diſſolved by a ſecreted liquor, or any other, for that purpoſe, ſurely a muſcle, &c. muſt undergo the ſame preparatory proceſs. Yet, by the by, why are not the lymphatics as capable of removing or deſtroying bone as the arteries are of forming it? There is as much reaſon in the belief of the one as of the other; and, from the number, variety, and importance of the facts in our poſſeſſion, every perſon muſt, in the end, admit both, although he may not underſtand either.

That bone is a ſecretion from the arteries, is inferred from a variety of circumſtances. This opinion, I believe, is ſaid to have originated from the late Mr. Hunter,† as well as the

* Monro on the Brain, the Eye, and the Ear, p. 51. Cruikſhank, p. 101.
† Anatomy of the Abſorbing Veſſels, by Mr. Cruikſhank, p. 102.

abſorption of bone; of which laſt, the preſent Dr. Monro has endeavoured to eſtabliſh himſelf the author. But if you examine a Treatiſe on Human Oſteogeny, by Dr. Neſbitt, you will find the firſt of theſe two opinions advocated in a very able manner. This ingenious man, who wrote and publiſhed, in the year 1736, what he had read in the anatomical theatre at London, in 1731, which was before the birth of Mr. Hunter, ſays he detected minute particles of bone in the extremities of the arteries, near the place of their deſtination. He ſuppoſes that ſmall veſſels, peculiarly appropriated for the purpoſe of oſſification, paſs off from arterial branches, and are continued on into the beginning of the veſſels "prepared to receive refluent juices," as he expreſſes himſelf; by which veſſels I ſuppoſe he means the returning veins. The "bony corpuſcles," to uſe his own words, are formed in, and depoſited by, theſe peculiar veſſels, in parts which are intended to be bone. That bone is not derived from the condenſation of cartilage, as is believed by ſome, is evident from the fact, that cartilage does not contain any calcareous earth. The origin of this opinion is probably from the circumſtance of bone being oftener formed in cartilage than in other parts. But it is not peculiar to cartilage. I once diſſected a man, in company with the late Dr. Cooper, one of whoſe teſtes was largely oſſified. Dr. Donald Monro relates* the diſſection of a woman, in whom a number of the meſenteric glands were found oſſified. A number of ſimilar obſervations we find in Schenkius.† Mr. Watſon diſcovered,‡ in diſſection, that part of the heart which lies upon the diaphragm oſſified; and ſays he has in his poſſeſſion a preparation, in which oſſeous matter is ſcattered over both heart and pericardium. A more ſtriking inſtance of the oſſification of the heart is mentioned by Mr. Gueſt.§ Inſtances of the oſſification of the arterial ſyſtem, in old people, are ſo familiar that it would be needleſs to mention one. The figure of a bone is deſcribed by Mr. Cheſelden, which was taken from the falx of the dura mater of a man who died of head-ach.‖ A ſmall oſſification was found on the inſide of the dura mater, near the falciform proceſs, in old Bayles, who was ſuppoſed to have been 130 years old,

* Tranſact. Lond. College, vol. ii. No. 18.
† De Meſenterio, p. 445.
‡ Medical Communications, vol. i. No. 19.
§ Medical Muſeum, vol. iii. No. 22.
‖ Jones' Abridg. of the Philoſ. Tranſact. vol. v. p. 202.

by Dr. Keil.* A ſimilar caſe is mentioned by Mr. Paiſley.† Even the brain itſelf has been found oſſified; a caſe of which is mentioned by Duvernev, in Memoires Acad. des Sciences for 1703, as quoted by Whytt.‡ Mr. Henry has recorded§ the caſe of William Carey, part of whoſe muſcular ſyſtem took on a ſurpriſing proceſs of oſſification. The oſſification commenced at his wriſts and ankles, and continued up to his elbows and knees, ſo as to put on the appearance of compact maſſes of bone. He had no uſe of the parts.—So much diſpoſed is the animal body to become oſſified, that we find, in the Philoſophical Tranſactions,‖ an account, by Dr. E. Tyſon, of the tendons of a fowl, the external membrane of the liver of a human body, as well as the outſide of the ſpleen and lungs, having been found in a ſtate of oſſification.

Many deviations of this kind, from the regular courſe of nature, in the formation of bone, might be adduced; but thoſe I have mentioned are ſufficient to ſhow that cartilage is not the excluſive ſeat of oſſification. The bones of the cranium are originally formed between membranes. Neither does bone depend upon the preſence of perioſteum for its formation, as Du Hamel maintained: for not to mention that all our bones almoſt are produced before the perioſteum is, they will form a callus, and be renewed where their natural covering is abraded. The fact appears to me to be ſtrictly this: When a part undergoes the oſſific proceſs, it only ſerves as a bed or matrix for the bony fibres to ſhoot in, and be ſupported.¶ It is very probable that the cartilage is better ſuited to this purpoſe than any other ſubſtance in the body. During the proceſs of oſſification, the parts in which it takes place diſappear, in conſequence of being abſorbed as faſt as the bone is formed, and is capable of ſupporting itſelf alone. This is proved by cutting an oſſifying cartilage into pieces, and macerating them in water till the blood-veſſels rot, and the portion of bone which has been formed will drop out ſpontaneouſly, leaving a vacuity in the cartilage, correſponding exactly to the ſize of the bone.

* Jones' Abridg. of the Philoſ. Tranſact. vol. v. p. 345.
† Med. Eſſays and Obſ. of Edin. Soc. vol. ii. No. 21. 12mo.
‡ Eſſay on Vital and Involv. Motions. 8vo. p. 8.
§ Medical Muſeum, vol. i. No. 3.
‖ Lowthrop's Abridg. vol. iii. p. 15.
¶ As the coagulable lymph of the blood does for the elongation of the arteries.

Another proof that the arteries are the inſtruments of oſſification is, that immediately before bone is formed in a part, that part uniformly becomes very vaſcular. This is very remarkable in the cartilage of the patella.—I am ſo much convinced of the truth of this doctrine, that I am diſpoſed to extend it to the production of other parts as well as bone. Indeed, without the arteries are the agents by which all parts of the animal machine are generated, I am ignorant of the manner in which their origin is to be explained. Ever ſince the wonderful injections of the celebrated Ruyſch were performed, reſpect for this doctrine has been increaſing daily; and the arterial ſyſtem is now conſidered the moſt eminent in its nature, and the moſt important in its function, of any other ſyſtem in the body. The extreme vaſcularity of glands has induced phyſiologiſts to believe that the ſecretion of fluids is performed by veſſels. The chyle does not poſſeſs the properties of blood until it has been acted upon by the blood-veſſels, although it may have been partly animalized by the lacteals; and it is from the conveyance of this blood, when formed by the veſſels which contain it, that every part of the body derives nouriſhment, regeneration, and an extenſion of life.

There can be no doubt that the filling up of cavities by granulation depends ſolely upon an elongation of veſſels: for, if an injection be thrown dexterouſly, and with ſome force, into an artery which paſſes to an ulcer, the injection will ooze out upon the ſurface of the granulations. In the ſame manner, I believe, it is that the renewal of every part of the body is effected—bone, integument, tendon, cartilage, &c. as well as the veſſels themſelves. But it is a circumſtance worthy of remark, that the ſame veſſels do, ſo wiſely and diſcriminately as it were, ſecrete ſubſtances ſo totally different in their nature and uſe as theſe juſt mentioned, according to the calls of nature. But we find this is not always the caſe; for inſtances have been mentioned, where parts which were not intended to be bone, have become oſſified.

The laſt proof I will mention of the ſecretion of bone by arteries, is connected with inflammation, which is a fruitful ſource of inſtruction to us on this ſubject. The proximate cauſe of inflammation conſiſts in an increaſed action of the blood-veſſels. This action is entirely confined to the arteries, and not to the veins; and, in whatever part of the body it may exiſt, it is the ſame diſeaſe. This inflammation, unleſs ſubdued by art, is always terminated by the effuſion of ſome kind of matter, which effuſion is thrown out from the extremities

of the arteries. The nature of the effuſed matter varies according to the nature of the part into which it is thrown. This is particularly evidenced by the regeneration of parts. If a tendon is divided, the ends of it are agglutinated together by the effuſion of tendinous matter from the inflamed arteries. The knitting of broken bones depends upon the ſame principle. In the venereal, and other diſeaſes, the bones are ſometimes attacked with pain, heat, and other ſymptoms of inflammation, which, in the ordinary time, are terminated by the effuſion of bony matter, which conſtitutes nodes and exoſtoſes.

From a view of what has been ſaid, it would ſeem that the arterial and lymphatic ſyſtems act in oppoſition to each other during life, and that, when one takes on action paramount to that of the other, diſeaſe is the conſequence. It is from the irregular action of the arteries, moſt commonly, that diſeaſe ariſes, as it is but ſeldom the lymphatics abſorb an undue quantity of matter. Of the two ſyſtems of veſſels, the abſorbents appear to be the moſt regular in their function. Thus, whilſt we obſerve the arteries evolve the different parts of the body, in an abundant, and ſometimes in an indiſcriminate manner, the lymphatics mould, or give them their proper figures, by carrying off the ſuperfluous and miſplaced arterial productions: and it is to the abſorbents alone that man (in whom elegance of ſtructure is ſo prominent) is indebted for the form of his bones, and for the general configuration of his body.

Mr. Hunter ſuppoſes that the abſorbents have a power ſimilar to that of a caterpillar eating up the leaf of a tree; and believes that they elongate themſelves, or ſhorten themſelves, as their object recedes or approaches nearer. But this opinion is leſs ſatisfactory than ſome others: for who can conceive of a ſoft veſſel, ſuch as an abſorbent, being able to tear away the hard ſubſtance of bones? No: ſuch an opinion as this cannot laſt long; and although I cannot broach a more plauſible and inviting doctrine, I muſt content myſelf, at preſent, with the following remark, which appears to be ſo well grounded, that I ſuſpect it will outlive all the theories to be formed for an age to come: The actions of the abſorbents are as much *forced* as thoſe of any other parts of the animal body. The ſtimulants, which ſupport and keep in action the powers of the abſorbents, are numerous, and may be either of a mechanical or ſpecific nature. Preſſure is the moſt common mechanical ſtimulant to the abſorbents

hence we diſcover the traces of it upon the bones. The deſtruction of ribs, in the caſe of James Tarney, was probably owing, in the firſt inſtance, to the preſſure of the tumour upon them. A caſe is mentioned by Dr. Donald Monro,* in which the preſſure from a tumour of the brain made a hole in the os frontis. Mercury, digitalis, ſquills, and even the contents of the abſorbents, are all particular or ſpecific ſtimuli to them; and, unleſs ſome of theſe ſtimulants influence the lymyphatic ſyſtem, it becomes inactive, which ſtate will be uniformly indicated by diſeaſe.

ARTICLE VIII.

OBSERVATIONS *on the* HAIR-WORM *(Gordius Aquaticus Lin.?) by Mr.* GEORGE CHILTON, *Teacher of Geography, Aſtronomy, &c. in New-York: In a Letter to Dr.* MITCHILL, *dated April* 28, 1800.

DURING my reſidence in the country, towards the latter end of autumn laſt, I met, for the firſt time, with the water-worm, known among the country people by the name of hair-ſnake. Curioſity, together with the common notion of its being produced by the action of the ſun upon a horſe-hair ſteeped in water, induced me to examine it; for which purpoſe I confined it in a baſon of water for the ſpace of a week or more; during which time I was not a little gratified with the activity it diſcovered in ſwimming about, but could perceive no change reſpecting it, except that it grew languid, or leſs lively, which I ſuppoſed to be owing to a want of freſh water.

Shortly after this I procured another from a ſpring-bath, in a field, which I ſecured alſo for the purpoſe of examination, by putting it in a baſon of water. After amuſing myſelf with it a while, I left it. On my return, after being abſent about half an hour, I was ſurpriſed with the ſight of ſomething in the act of proceeding from one of its ends, reſembling a piece of very fine white thread. I immediately began to conjecture that it might be one of its young. It ſeemed in length about half an inch. In the courſe of an hour it made its way about half an inch further. I left it in this ſituation,

* Tranſact. Lond. Col. vol. ii. p. 353.

and returned in about an hour, when I found it wholly expelled, lying coiled up at the bottom of the baſon. Expecting to find ſome ſymptoms of life about it, I examined it with all the minuteneſs I was capable of, but was diſappointed. On taking it out of the water, I found it exceedingly tender; but not having a microſcope, could make nothing more of it. Putting it in the water again, I left them for the ſpace of an hour, when, on my return, I found another one proceeding in the ſame manner as the former, which, in the ſpace of an hour, was alſo wholly expelled.

In this gradual manner was the expulſion continued till the next day, when I found the number amounted to about twelve, which were ſeemingly of an equal length, viz. two and a half inches; for they were ſo twirled about each other, and ſo very tender, it was not eaſy to determine, with accuracy, either their number or their length.

They were ſuffered to remain in the baſon about a week, during which there was no perceptible alteration.

Being at the bath ſhortly after this, I obſerved another of thoſe creatures to ſwim from a projecting part of its edge. I endeavoured to take it; but the bath being wide, it ſheltered itſelf among ſome weeds on the oppoſite ſide, and eſcaped. On examining the part from which I had obſerved it to ſwim, I was pleaſingly ſurpriſed with a view of a bunch of theſe young ones (for ſuch I took them to be) twiſted around the root of a weed, which hung down into the water from a part that jutted over. On a cloſe inſpection, without diſturbing either them or the water, I was able to diſcover life in two or three of them, which were a little larger than the reſt. They were faſtened at one end to the bunch, but with the other moved freely in the water. A ſmall black ſpot was likewiſe obſervable on the moveable end; but, in other reſpects, they were ſimilar to thoſe I had in the baſon. Leaving them in this ſituation, and returning the next morning, I was not a little aſtoniſhed to find that the old one had returned, and was twined about the young ones in ſuch a manner as ſeemed to indicate the exiſtence of a principle ſomewhat ſimilar to natural affection.

On looking along the edge of the bath, I found another bunch of young, with the old one, twined about a root in the ſame manner. After viſiting them occaſionally for a few days, without obſerving any material alteration, they became torpid, in conſequence of the increaſing cold of the ſeaſon, in which ſtate I left them to take their winter's repoſe.

ARTICLE IX.

ARRANGEMENT *of* FACTS CONCERNING ULCERS, SORES *and* TETTERS; *ſhowing how agreeably theſe and ſimilar Affections of the Skin are healed, in many Caſes, by* ALKALINE APPLICATIONS: *In a Letter to* THOMAS TROTTER, *M. D. Phyſician to the Britiſh Fleet, &c. dated New-York, September* 20, 1800.

FROM the great benefit derived to mankind by alkalies and ſoaps, and particularly from the pleaſant effects wrought by them upon the ſkin, it would ſeem that the whole of their operation, when thus applied, would have been underſtood ages ago. Yet the facts, plain and obvious as they are, have not, as far as has come to my knowledge, been duly noticed. The *cauſtic* effects of the ſimple alkalies have been duly obſerved. The corroſiveneſs of ley, made from aſhes juſt taken from the heated hearth, or draining from a leach-tub partly filled with quick-lime, has been a long time familiar to waſhers and bleachers of linen and other cloths. They knew very well that the ley might be made ſtrong and ſharp enough to eat away the ſkin and fleſh of their hands, and to deſtroy the firmneſs and coheſion of the goods. And from theſe accidents ſurgeons ſeem to have taken the hint of preparing ALKALINE CAUSTICS, which are generally little more than *naked pot-aſh*, or the vegetable fixed alkali deprived of its carbonic acid by quick-lime.

Cauſtic pot-aſh, thus, having been much employed in domeſtic economy, and ſometimes in ſurgery, was generally looked upon as a fearful and formidable thing. As it was dangerous to be touched or taſted, there was good reaſon for popular uneaſineſs whenever it was brought in contact with the living body. In that form it was capable of doing great damage. Hence have proceeded, in all probability, the alarming ſtories about the corroding, acrimonious, attenuating, diſſolving and putreſcent qualities of alkalies, found in ſuch writings as thoſe of HUXHAM, and in various other popular books. They ſeem, in thoſe ways, to have got a bad character, which they have never wholly retrieved.

Acids, when united with alkaline ſalts, diminiſh remarkably their keenneſs and pungency. Theſe neutral compounds are, therefore, by a great difference, more mild and manage-

and returned in about an hour, when I found it wholly expelled, lying coiled up at the bottom of the baſon. Expecting to find ſome ſymptoms of life about it, I examined it with all the minuteneſs I was capable of, but was diſappointed. On taking it out of the water, I found it exceedingly tender; but not having a microſcope, could make nothing more of it. Putting it in the water again, I left them for the ſpace of an hour, when, on my return, I found another one proceeding in the ſame manner as the former, which, in the ſpace of an hour, was alſo wholly expelled.

In this gradual manner was the expulſion continued till the next day, when I found the number amounted to about twelve, which were ſeemingly of an equal length, viz. two and a half inches; for they were ſo twirled about each other, and ſo very tender, it was not eaſy to determine, with accuracy, either their number or their length.

They were ſuffered to remain in the baſon about a week, during which there was no perceptible alteration.

Being at the bath ſhortly after this, I obſerved another of thoſe creatures to ſwim from a projecting part of its edge. I endeavoured to take it; but the bath being wide, it ſheltered itſelf among ſome weeds on the oppoſite ſide, and eſcaped. On examining the part from which I had obſerved it to ſwim, I was pleaſingly ſurpriſed with a view of a bunch of theſe young ones (for ſuch I took them to be) twiſted around the root of a weed, which hung down into the water from a part that jutted over. On a cloſe inſpection, without diſturbing either them or the water, I was able to diſcover life in two or three of them, which were a little larger than the reſt. They were faſtened at one end to the bunch, but with the other moved freely in the water. A ſmall black ſpot was likewiſe obſervable on the moveable end; but, in other reſpects, they were ſimilar to thoſe I had in the baſon. Leaving them in this ſituation, and returning the next morning, I was not a little aſtoniſhed to find that the old one had returned, and was twined about the young ones in ſuch a manner as ſeemed to indicate the exiſtence of a principle ſomewhat ſimilar to natural affection.

On looking along the edge of the bath, I found another bunch of young, with the old one, twined about a root in the ſame manner. After viſiting them occaſionally for a few days, without obſerving any material alteration, they became torpid, in conſequence of the increaſing cold of the ſeaſon, in which ſtate I left them to take their winter's repoſe.

ARTICLE IX.

ARRANGEMENT *of* FACTS CONCERNING ULCERS, SORES *and* TETTERS; *ſhowing how agreeably theſe and ſimilar Affections of the Skin are healed, in many Caſes, by* ALKALINE APPLICATIONS: *In a Letter to* THOMAS TROTTER, *M. D. Phyſician to the Britiſh Fleet, &c. dated New-York, September* 20, 1800.

FROM the great benefit derived to mankind by alkalies and ſoaps, and particularly from the pleaſant effects wrought by them upon the ſkin, it would ſeem that the whole of their operation, when thus applied, would have been underſtood ages ago. Yet the facts, plain and obvious as they are, have not, as far as has come to my knowledge, been duly noticed. The *cauſtic* effects of the ſimple alkalies have been duly obſerved. The corroſiveneſs of ley, made from aſhes juſt taken from the heated hearth, or draining from a leach-tub partly filled with quick-lime, has been a long time familiar to waſhers and bleachers of linen and other cloths. They knew very well that the ley might be made ſtrong and ſharp enough to eat away the ſkin and fleſh of their hands, and to deſtroy the firmneſs and coheſion of the goods. And from theſe accidents ſurgeons ſeem to have taken the hint of preparing ALKALINE CAUSTICS, which are generally little more than *naked pot-aſh*, or the vegetable fixed alkali deprived of its carbonic acid by quick-lime.

Cauſtic pot-aſh, thus, having been much employed in domeſtic economy, and ſometimes in ſurgery, was generally looked upon as a fearful and formidable thing. As it was dangerous to be touched or taſted, there was good reaſon for popular uneaſineſs whenever it was brought in contact with the living body. In that form it was capable of doing great damage. Hence have proceeded, in all probability, the alarming ſtories about the corroding, acrimonious, attenuating, diſſolving and putreſcent qualities of alkalies, found in ſuch writings as thoſe of HUXHAM, and in various other popular books. They ſeem, in thoſe ways, to have got a bad character, which they have never wholly retrieved.

Acids, when united with alkaline ſalts, diminiſh remarkably their keenneſs and pungency. Theſe neutral compounds are, therefore, by a great difference, more mild and manage-

able than the naked alkalies. And thus it is that ſeveral of them poſſeſs, at the ſame time, the virtues of ſimple alkalies, without their inconveniences.

It is known among farmers and dealers in horſes in America, that, when the ſkin of theſe animals is chafed by the collars, or galled by the ſaddles, a good application to the parts ſo injured is a weak alkaline lixivium, made by putting ſome common wood-aſhes in water. The waſh diſpoſes the ſore to heal quickly. This application is commonly a carbonate of pot-aſh.

The white fine powder remaining after the combuſtion of *hickory-coals* (juglans) has been deemed more neat and efficacious than common aſhes from other ſpecies of wood, and applied, they ſay, very advantageouſly to relieve horſes injured as before deſcribed.

Tobacco-aſhes have been recommended as very efficacious in removing that tetter or eruption called "the ring-worm." This too owes its activity to the alkaline ſalt with which it abounds.

Obſtinate eruptions and ſores about the mouths, lips, and chins of children, have been cured, in country places, by being ſprinkled by their mothers with the nice and light aſhes remaining after the conſumption of hickory wood by fire.

Lime-water has long had a place in the materia-medica; and, beſides its internal uſe for urinary calculus, and diſorders of the alimentary canal, has often been employed, with advantage, for ulcers on the legs and other parts of the body.

Indeed, the aqua phagedænica, made by diſſolving corroſive ſublimate in lime-water, is little elſe than a plain ſolution of alkaline or calcareous earth in water; for the lime immediately unites with a portion of the muriatic acid, into a muriate of lime, and the quick-ſilver, turned to a yellowiſh oxyd, falls to the bottom in the form of a powder. I conſider the chief operative ingredient in this pharmaceutical preparation to be the lime-water.

Pliny mentions (Hiſt. Nat. lib. xxiii. Proœm.) the healing qualities of the *aſhes of the vine* (pot-aſh), in chaps, fiſſures, piles, excoriations and eryſipelas; and relates that (ibid. lib. xxiv. cap. 6.) the aſhes of pine and larch-tree bark are good for the places fretted and galled between the thighs, and for healing burns and ſcalds. The ſame author recommends the aſhes of burnt ſponge, which conſiſt chiefly of ſoda with carbonic matter, for diſeaſes of the eyes and eye-lids, and for ſeveral other complaints (ibid. cap. 11.); and he affirms (ibid.

lib. xxi. cap. 10.) that ſoda is good againſt the leproſy, and unſeemly ſpots and freckles which blemiſh the ſkin, with other remarkable particulars to the ſame point.

I knew that for ſpongy and fœtid gums, accompanied with bleedings, and with naſtineſs about the teeth, weak alkaline ſolutions were of ſovereign efficacy to cleanſe the mouth, to leſſen the fœtor of the breath, and to diſpoſe the diſeaſed parts to become ſound and healthy. I have, for ſeveral years, been in the habit of preſcribing them to remove the ſordes accumulated there in ſyphilis, ſcurvy, fevers, and, in ſhort, in all other caſes where the mouth had collected filth, and ſtood in need of being cleaned out.

Taught by ſuch facts, I began to think that the purulent ſecretion in ſores, wounds and ulcers, underwent ſome other change than a mere thickening of its conſiſtence, while expoſed on their ſurface. There was the higheſt probability that a portion of oxygenous air underwent decompoſition, and that its baſis, attracted by the pus, imparted to it the qualities of an acid. And from the ſtimulating, corroſive, or other morbid operation of this acid upon the ſubjacent parts, many of the phenomena of ulceration might tolerably well be accounted for.—Chemiſtry did not here meddle with *living parts or functions.* Its principles were only applied to explain ſome of the phenomena of animal fluids, wherein *no vital energy of any ſort was preſumed to inhere.* Fluids, *after ſecretion*, have ever been deemed fair ſubjects of chemical analyſis.—As I had, long before, been led to believe, that intermitting, remitting, and continued fevers, in all their modifications, were but the effects which oxygenated ſepton wrought upon the body, I was naturally led to a perſuaſion, that the fevers conſequent upon deep and extenſive ulceration, were aſcribable to a ſimilar cauſe. Thus the hectic paroxyms accompanying vomica of the lungs, conſequent upon the admiſſion of air into the purulent cavity of pſoas abſceſs, and attendant on broad ulcerated and expoſed ſurfaces of every ſort, apparently aroſe from an abſorption of ſeptic acid, formed on the denuded ſpots, by a combination of a portion of atmoſpheric oxygen with the azotic material of the ſecreted humour. This, I thought, approached to ſomewhat of a rationale of *hectic* chills and heats, by referring them to the ſame general law of nature which governed the phenomena of *other* fevers. I have endeavoured to ſhow, in other places (particularly in my piece on dyſentery, ſoon to be publiſhed), that the *ſeptic poiſon* which produces febrile action is often

formed in the alimentary canal, from the degenerated remains of food: it looked now almoſt as likely that *virus, of nearly the ſame compoſition*, was formed on the ſurface of ulcers, from the fluids effuſed there. I ſuſpect ſome of the phenomena mentioned in your chapter on malignant ulcer, in the ſecond volume of your Medicina Nautica, proceed from the ſeptic acid converted to a gaſeous form, and becoming infectious. And I intend this letter as a ſequel to that chapter.

This concluſion was ſtrengthened by conſidering the qualities of the pus in many caſes of ulceration. It ſeemed to devour the contiguous parts, and ſometimes to eat away all before it, conſtituting what you have deſcribed ſo well. The exhalations from this deſtructive liquid were oftentimes noxious as well as offenſive; and wherever they were abundant enough, as in the wards of military hoſpitals, and on board of ſhips, they evidently acted as the exciting cauſe of "fevers" among thoſe who were confined in the ſame apartments. Here the ſeptic effluvium of putrid ulcers was productive of conſequences reſembling thoſe ordinarily occurring from the operation of ſimilar vapours from other ſources of corruption.—It was, however, ſcarcely to be doubted, though this kind of acidity was the moſt common, that, occaſionally, *ſome other than the ſeptic acid* was the morbid product.

Under the conviction that ALKALINE REMEDIES were fully indicated as topical applications in the practice of ſurgery, I ventured to try them in a number of caſes which fell under my care. The firſt inſtance was the venereal chancre removed by *carbonate of pot-aſh*, which is related at large in Med. Rep. vol. ii. p. 217—218, firſt edition. Since which it has been in my power to ſee many other, and more ſerious ſyphilitic ulcers, of the primary kind, yield to the ſame alkaline treatment. Indeed, in incipient chancres, the deliqueſced carbonate of pot-aſh (lixivium tartari) is my ſteady preſcription. I never knew bubo ariſe from its application, as frequently follows the uſe of ſeptite of ſilver (lunar cauſtic). It does not, like that metallic ſalt, deſtroy life and organization, nor cauſe the part to mortify and ſlough away; but it preſerves a degree of cleanlineſs unknown under any other dreſſing. Some caſes, which afterwards occurred under Dr. Rodgers and myſelf, were extracted from the books of the New-York Hoſpital by Mr. Hayes, then the apothecary of that houſe, and publiſhed in the third volume of the Med. Rep. p. 302 & ſeq.

Emboldened by theſe inſtances of ſucceſs, I directed *ſecondary* ſyphilitic ulcers to be treated with alkaline applications. When

in their very foul ſtate, the fluid carbonate of pot-aſh was applied by aid of a camel's-hair bruſh, and repeated from time to time, until the ſore became more clean, which commonly happened in a few days. Afterwards, lint dipped in lime-water, and kept moiſt with the ſame, was the common dreſſing; the ſore being touched, from time to time, with the carbonate of pot-aſh, if deemed neceſſary.—In other caſes, I ordered the ulcers, after having been duly alkalized, to be dreſſed with an ointment, made by rubbing together, upon a tile, carbonate of ſoda and the cerate of wax and oil. This was ſpread upon lint, and applied in the common manner. Sometimes a poultice was directed, and ſometimes not.

In caſes of ulcerated buboes, and of venereal blotches about the ſkin, I have cauſed dreſſings of the ſame kind to be made uſe of; and I approve of them, and thoſe herein mentioned, upon the whole, more than any others with which I am acquainted.

In one inſtance I was agreeably ſurpriſed by a very unexpected occurrence. A girl, whoſe conſtitution had been conſiderably injured by ſyphilis, was affected, in addition to diſeaſe of the genital parts, with an ugly and ſcabby eruption over her face and neck, and with an enlargement and inflammation of the lachrymal ſack of the right eye. Determining to defer the particular treatment of the fiſtula lachrymalis until the general and more urgent diſeaſe of her conſtitution ſhould have abated, I preſcribed, beſides other things, a weak ſolution of carbonate of ſoda in water, as a lotion for her face. This had the uſual effect of diſpoſing the eruptions to dry away, and diſappear in a few days: but, what I had not at all expected at the time, the *fiſtula lachrymalis diſappeared too*, under the continuance of the alkaline waſh, and returned no more.

I am of opinion that, in old ſyphilitic caſes with ulceration, the ſick actually ſuffer a twofold diſtemper; one part of which was brought on by the original virus, and the other proceeds from the oxygenated pus conſtantly abſorbed from the ſurface of the ſores. By alkaline applications, this latter ſource of miſchief is leſſened, and the acidity of the pus is overcome; the *local irritation* abates, and the *conſtitutional infection* by it ceaſes; and thus, as the ulcers heal, the febrile ſtimulus which uſed to be produced there is proportionally diminiſhed; and when they become cicatrized, they furniſh not a particle of it any longer.—Latterly I became ſo convinced of the diſpoſition to heal induced by alkalies, that I

ordered ſeveral deep and ſinuous ulcers to be dreſſed with lint dipped in the fluid carbonate of pot-aſh: and the reſult here was no leſs agreeable than in the other caſes.

Thus far I had proceeded, when, on reading the Medical and Phyſical Journal for December, 1799, I obſerved that a gentleman, diſtinguiſhed for his poetical as well as profeſſional talents, Dr. Drake, had quoted Brugman's Diſſertation on Pus, proving it *to be an acid.* Theſe experiments, which I had never ſeen nor had any knowledge of until then, I ſhould be glad to peruſe. The eſtabliſhment of this one fact fixes a principle of great moment in pathology, and enables us to practiſe upon ulcers, and the fevers which accompany them, upon ſomething like a conſiſtent and rational plan; and, more than this, puts it in our power to compare the phenomena of fever from the abſorbed acid of an ulcer, with fever from the imbibed acid of dyſenteric inteſtines, and the fever from the inhaled acid ariſing from ſubſtances putrefying in the open air; and to refer them to a general law of the animal economy, whereby *oxygenated ſepton*, acting as a poiſon, and known ſometimes under the names of contagion, infection and miaſma, ſtirs up thoſe varieties of commotion in the living frame.

It would be highly worthy of your official character, and wholly accordant to your various and benevolent plans for bettering the condition of ſeamen, to recommend alkaline dreſſings, among other things, for old and offenſive ulcers, as the navy-ſurgeons would have great opportunities of trying them, and of witneſſing their good effects.—Hoping and labouring earneſtly for the improvement of medicine and ſurgery, I am aſſuredly, &c.

SAMUEL L. MITCHILL.

REVIEW.

ART. I. *Memoir concerning the Diſeaſe of Goitre.* By Benjamin Smith Barton, *M. D. &c.*

[Continued from page 53, and concluded.]

WE proceed, according to our promiſe, to give an account of FODERE's publication on Goitre and Cretinage. FODERE's work, the dedication of which has been accepted by the Royal Academy of Sciences of Turin, is divided into four ſections. In the firſt, the author deſcribes the Goitre, its ſeat, differences, cauſes, and the remedies which phyſic and ſurgery may uſe for its cure. In the ſecond he ſpeaks of the complete Cretinage, its differences, propagation, and particular cauſes. In the third he inquires into the general cauſes of the Goitre and Cretinage, and the reaſon why theſe diſeaſes are endemic in his country (the province of Maurienne). The fourth ſection contains the phyſical and moral means by which Goitre and Cretinage may be extirpated from the ſubalpine valleys; and it is ended by ſome particular and philoſophical conſiderations on the province of Maurienne.

The deſcription of the thyroïd gland, as the ſeat of the tumour commonly called *bronchocele*, comes, in the firſt place, under our conſideration. That gland, ſays he, contains a humour of the ſame nature as thoſe deſtined to lubricate all the parts of the body, the deſication of which might undergo ſome alteration. The collection of that humour, in conſiderable quantity, in the gland, when choaked up by any cauſe, produces the Goitre. He gives a muſcle *azygos* to that organ: he calls it conſtrictor; and, as it covers the gland, it exerciſes a preſſure upon it; ſo that he conſiders the thyroïd as a conglomerate gland, whoſe ſecretion moiſtens and lubricates the larynx and *trachea arteria*, continually exſiccated by the air neceſſary to reſpiration. His anatomical experiments are not carried ſo far as we ſhould expect from ſo intereſting and extenſive a work. On that ſcore he does not agree with many anatomiſts, who conſider that gland only as a mucous one, quite deprived of any excretory duct, and immediately covered by the muſcles called *ſterno-hyoideus*, *ſterno-thyroi-*

deus, and *omoplato-hyoideus*, ſtrongly connected together by a kind of aponeurotical expanſion, without an *azygos* muſcle.

The details he gives of the cauſes are delivered with great preciſion and accuracy. They are to be found neither in the drink of ſnow-waters, nor in ſelenitous nor metallic waters, nor in coarſe, thickening and inſpiſſating food, *but in the warm dampneſs intimately diffuſed in the atmoſphere of the deep valleys (which he calls ſubalpine), by which the inhabitants are conſtantly ſurrounded.*

Living at St. Jean de Maurienne (a town ſituated on the plain, where the Goitres and Idiots are not ſo common), the author has been ſo careful as to go and explore that country, at repeated times, as far as was neceſſary: ſo that, beſides his tables of compariſon between the inhabitants of the valleys, thoſe of the plain, and thoſe living on the declivity of the mountains, his proofs on this head are reinforced and illuſtrated by various reaſoning. It is known that that country exhibits more people with big throats than any other in the world, as all the cauſes act more powerfully, and are more concentered and combined, in the deep valleys of Maurienne, Savoy, Valais, &c.

They whoſe habitations are encompaſſed with trees, furniſhed with large leaves, and eſpecially fruit trees, or ſituated in the neighbourhood of rivers, torrents, lakes, ponds, ſwamps, and expoſed to the currents of ſouth and weſt winds, are more particularly affected with thoſe infirmities and tumours. He has always obſerved the Goitre make its appearance in the ſpring, when the trees are adorned with leaves, and to diminiſh in the beginning of winter, when they caſt them off. It diminiſhes more when the winter ſeaſon is cold and dry, and *vice verſa*.

He believes that there are no miaſmata nor foreign particles diſſeminated in the air ſufficient to give riſe to the Goitre. "If there are any miaſmata, if truly they are exiſting, they muſt be very light, and, of courſe, carried upwards with the columns of the air that we breathe, as the azotic gas is."

The number of big throats diminiſhes as you advance from the low grounds towards the high places, the level of mountains and open plains, and are entirely extinct, except among thoſe who have emigrated, on high and naked places. A great many other illuſtrations are unavoidably paſſed without notice. Our author expatiates pretty lengthily on the relaxation of the ſoft parts, and its effects. From the permanent dampneſs of the atmoſphere, the fibres of the bodies are relaxed by degrees,

the *vis tonica* diminiſhed and weakened, the perſpiration checked, an accumulation of mucous humour in the thyroïd gland takes place, and, of courſe, the enlargement of that gland, and the formation of the Goitre.

The author acknowledges that the Goitre is often hereditary, and the *Cretins*, or ideots, are, at firſt, more commonly afflicted with a ſmall Goitre. "Many writers have confounded the Goitre with ſcrophula. *Heiſter*, *Riolan*, and *Mittermeyerus*, have ſet the Goitre in the ſame chapter with it. Yet we ſhould be much miſtaken if we made no difference, although theſe diſeaſes have ſome common ſymptoms—the principal of which are, a weak conſtitution, white, fine and delicate ſkin, blue eyes," &c. He gives a ſtriking difference between the two diſeaſes, which are ſometimes found exiſting in the ſame individual. "The ſcrophula," ſays he, "ſeems to exiſt in all the ſolids weakened, and eſpecially in the whole glandular ſyſtem. The infant has a haggard eye, and the upper lip thicker than uſual. It ſeldom makes its appearance before two years of age, and ſcarcely ever after ten or twelve. It diſappears ſometimes at the age of puberty, and on the approach of the menſtrual flux. The Goitre, on the contrary, is a local diſeaſe of the throat, which often ariſes at the birth, or ſome time after. The moſt common epoch is at ſeven, eight, nine and ten years; but it may take place at any age; and, when it once comes out, puberty does not diſſipate it.—Although nature has cured, now and then, a few perſons affected with true ſcrophula, it does not appear that art has yet done it. Art, on the contrary, radically cures Goitre, which nature alone does not perfectly."—Theſe paſſages, in our opinion, admitting no abridgement, we have thought proper to tranſlate them entire.

Dr. Foderè proceeds, in the next place, to deliver an account of his method of cure. The remedies conſiſt in an electuary, compoſed of ſponge half calcined, with honey, and powder of cinnamon, to be taken three times a day, to the quantity of a hazel-nut, till the tumour ſubſides; or thirty grains of hepar ſulphuris (ſulphure of pot-aſh), diſſolved in one quart of water, which ſhould be drunk every day.

Whichever may be the method of cure, he recommends a purgative previous to adminiſtering the above remedies, and a repetition of it every eight days, till the Goitre be cured. He obſerves, alſo, that the remedies ſhould be retained awhile in the mouth, and the patient kept warm and covered. Hav-

ing confidence in the influence of the moon, he adviſes to give the remedies towards its decline.

If children refuſe to ſwallow theſe medicines, frictions ought to be made on the tumours, either with a dry flannel aromatiſed, or with ſoap, or camphorated oil, as Underwood practiſed with ſucceſs. He recommends, alſo, to wear an oval and elaſtic plate of ſteel, as large as the Goitre, which being conſtantly fixed on it, exerciſes a ſufficient preſſure, and diminiſhes its ſize.

The author ſeems to be at a loſs to explain the effect of the remedies on the Goitre. When in the ſtomach, he aſks whether they operate immediately upon its nerves, or penetrate the ſyſtem of the circulation, and pervade the veſſels of the thyroïd gland? If it be the latter way, they muſt alkalize the blood, and the remedy is worſe than the diſeaſe.—Would it not be much better to recommend, once for all, and without heſitation, in the treatment of every Goitre, not to ſwallow any, or very little, of the medicines known to be the moſt efficacious—but to keep them, as long as poſſible, in the mouth, or to rub its inner parts with a proper and harmleſs powder? This point will be examined afterwards.

The author proceeds thus: "When the Goitre is old, ſcirrhous, and very large, it is uſeleſs to make uſe of any medicines. The operation alone may ſucceed, provided it be formed of a ſingle ſac, detached and hanging on the middle of the fore part of the neck, and narrower at its baſis than at the top. The ſkin muſt be divided by a crucial inciſion, and ſeparated from the tumour, which muſt be ſeparated and taken out. If there is an hæmorrhage, ligatures muſt be put on the veſſels, &c. In certain other caſes, a ſeton may be paſſed through the tumour, and is preferable to cauſtic, although the ſtring ſhould be anointed with ſome alkaline or mild cauſtic." He confeſſes to have been only a witneſs of two caſes in which the firſt operation was luckily and ſkilfully performed. Such is the whole method of cure that we have deemed neceſſary to detail.

In the ſecond part of the work, Dr. Foderè gives an hiſtorical and philoſophical deſcription of thoſe wretches, ſo common in the ſame valleys, called *Ideots* or *Cretins*, of whom Mr. *Coxe* and Mr. *De Sauſſure* have left us a ſtriking and lively picture; and he delivers his doctrine as a perſpicuous phyſician. He follows thoſe poor creatures, of whom the greater part are deaf and dumb, from their birth to their

death—divides them into Cretins, *perfect* or complete, and *imperfect*—diſtinguiſhes the perfect into ſix different ſtages—and, after having premiſed his theory, and inquired into the anatomy and phyſiology of their bodies, to ſubſtantiate it, and explain the immediate cauſes, he examines, with great accuracy, the prediſpoſing and remote cauſes of the *Cretinage*. Theſe principal cauſes are,

1ſt. An atmoſphere continually warm and damp, or cold and damp.

2d. A neglected and dirty, or too ſtudied and too effeminate a phyſical education.

3d. A moral education grounded on ridiculous and extravagant prejudices, or too elaborate, and beyond the capacity of thoſe ſubjected to it.

4th. The intercourſe of ignorant and ſuperſtitious people.

5th. Imitation which countenances an averſion to conſtant and regular labour.

6th. Continual gluttony.

7th. The abuſe of wine and ſpirituous liquors.

The topographical deſcription of the valleys, the ſtate of their atmoſphere, the relative connection of their dampneſs with the Goitre and Cretinage, fill almoſt entirely the third part of the work. The opinion of the author ſeems to derive ſome ſupport from chemical reaſoning and hygrometrical experiments. Previous to examining how far climate influences the mind and character of the inhabitants of the ſubalpine valleys, he undertakes to prove how the toughneſs of the brain (which is harder in the ideots than in other men) may be induced by dampneſs; and he begins with the following general propoſitions:

1ſt. The Cretinage (a few caſes excepted) is always hereditary; that is, a leſion of the brain (known to be the immediate cauſe) is already ſuppoſed in the father and mother. Thus an ideot child, or one who is to be ſo, is born with a beginning of preternatural hardneſs in the brain.

2d. It is probable that the Goitre has, at all times, preceded the Cretinage.

3d. The ideot's father and mother had a Goitre.

4th. A father, afflicted with a Goitre of a certain bigneſs, ſeldom begets children without ſome degree of Cretinage.

5th. A ſubject, affected with an extenſive Goitre from his youth, commonly becomes, after a great while, an ideot in ſome degree (the perfect ideot excepted), as related in the ſecond ſection. Thus, ſhould the Goitre give riſe to the Cretinage,

the humid air being the firſt cauſe of the Goitre, then it is likewiſe the firſt cauſe of the Cretinage.

The theory of the author is illuſtrated by an explanation of the formation of the hardneſs of the brain, from the effects of the atmoſphere on the veſſels, &c. He neglects nothing to ſearch into the cauſes of the diminution of both diſeaſes, in the valleys, for ſome years paſt; and, in order to proceed with more exactneſs, he has availed himſelf of the aſſiſtance of the curates of ſeveral pariſhes. "The travellers," ſays he, "who have paſſed through thoſe valleys twenty years ago, and croſs them again now, perceive a difference. Notwithſtanding this change, which actually exiſts, the number of ideots and big throats is ſtill conſiderable; ſince, in the Val-d'Aoſte alone, the number of perfect *Cretins* amounts to 1740, in a population of 68022 ſouls."

Having taken a haſty ſurvey of the three former parts, we ſhall now proceed to offer a curſory notice of the laſt. Many paſſages contain intereſting reaſonings, which we ſhall be, unwillingly, obliged to paſs over. Among the means propoſed to reduce and diminiſh the dampneſs of the atmoſphere, we ſhall take notice of the following:

1ſt. As the trees furniſhed with large leaves collect and preſerve dampneſs, the author recommends to cut down all the fruit-trees within the ſpace of four hundred paces around the houſes, inaſmuch as the fruits are noxious to the inhabitants of thoſe valleys, who ought alſo to remain unſhaded by any trees. With regard to the power poſſeſſed by the trees, of diffuſing dampneſs, he refers to BONNET's ſyſtem and experiments.

2d. To remove ſtanding waters, and drain the ſwamps annually filled up with the melted ſnows and overflowings of the rivers. Beſides the dampneſs reſulting from them, a prodigious quantity of inſects are bred and die in thoſe marſhes during the hot ſeaſon, and ſundry vegetables are putrified: hence ariſe an infectious air, and a great many intermitting fevers.

3d. It ſeems to him very important to raiſe the roads, which are now too low; to preſerve the greateſt cleanlineſs in the ſtreets of all the villages, and pave them; to erect houſes at a diſtance from rocks or high grounds, ſo as to give a free circulation to the winds. The people ſhould no longer inhabit their ſtables nor low rooms.

After having propoſed ſome means to render the human body leſs ſuſceptible, and abler to reſiſt the atmoſpheric damp-

neſs, Dr. Foderè relates how marriages ought to be regulated and determined by a committee, compoſed of the magiſtrate, the curate and the phyſician of the place, and what moral education it is requiſite to give the children of the valleys. To this laſt purpoſe, as the cure of the *perfect* Cretinage is impoſſible, he propoſes to proceed cautiouſly to the education of the *imperfect*, by the analytic method, which muſt be preferred to the ſynthetic—that is, from the moſt ſimple to the more complex. His means are wiſe, eaſy, and in conformity with his principles; and he quotes the authority of *Tacitus*, *Monteſquieu*, *Locke*, the immortal Abbe de *Condillac*, and Mr. *De Pauw*. He laments and deprecates the unheard-of and inconceivable ſlothfulneſs prevailing among the inhabitants of the towns and hamlets, who, at the ſame time, are inceſſantly reſtleſs and diſcontented. "There is a virtue," ſays he, "which ought to be early inculcated on the inhabitants of the valleys: it is the love of work. So true it is, that if governments wiſh the people to be happy and quiet, they ought to employ and keep them buſy. The man who works is fond of peace, and reſpects his duties. If not overwhelmed, he calmly devotes his days to labour, without being acquainted with the metaphyſics of government, and without diſquieting himſelf by thoſe vain diſtinctions which needleſſly teaze and weary ſo many men."

Finally, the work is concluded by propoſing ſundry means of improvement for the province of Maurienne: ſuch as a better adminiſtration concerning agriculture in general; the exploring of ſome mines of iron, copper and ſilver; the eſtabliſhment of good manufactures; to pay more particular attention to ſheep and wool, and to the tanner's art. He recommends the bark of fir-trees to make tan: as they grow in plenty in the mountains, and in the *high Maurienne*, they can get a great quantity of their bark. Thoſe trees ſtand and live four or five years after their being ſtripped. Many other important parts of the book admit neither analyſis nor abridgment: the philoſophical and patriotic views of its author cannot fail to reflect much honour upon him, and gain the grateful affection of his fellow-citizens.

However, we cannot take leave of this important ſubject without caſting a curſory view upon another and earlier work on Goitre, which was undoubtedly unknown to Dr. Foderè. The work we mean was ſent to the Academy of Surgery of Paris in the year 1789, which rewarded the author with a gold medal. That author is Louis Valentin, Doctor of Phy-

ſic in the Univerſity of Nancy, and lately chief phyſician of the French hoſpitals in Virginia. He ſeems to be the firſt who rejected the commonly-received opinion of attributing the Goitre to the uſe of melted ſnow, and ſelenitous or vitriolic waters; but he brings forward many proofs of the effects of cold and damp air, eſpecially during the night, upon a certain claſs of people, and military men ſubmitted to his care. We are told that Dr. Villars, of Grenoble, touched upon the ſubject of Goitre a little before Dr. Foderè; and he alſo acknowledges that ſnow-waters, and other cauſes to which the Goitre has been attributed, are unfounded. On that point, it is very ſatisfactory to find that Dr. Foderè agrees ſo well with the two other writers, without having been at all acquainted with their works. We underſtand that Dr. Valentin's memoir is not yet publiſhed.* We know, however, that he gives an anatomical and phyſiological deſcription of the thyroïd gland, which he conſiders only as a mucous and lymphatic one, quite deſtitute of excretory ducts. His method of cure is very different from Dr. Foderè's. It is a *ſweet* and *alkaline powder*, to be uſed at nights in bed, and ſometimes in the morning. It ſhould not be ſwallowed, but rubbed in the inſide of the mouth, and kept underneath the tongue, to be abſorbed by degrees, and conveyed by the lymphatics to the diſeaſed thyroïd gland. This is to be treated with warm and *alkaline* topics, ſuch as *warm aſhes*, *ammoniacal ſalts*, and waſhings with *brine*, *weak ley*, or a *ſolution of pot-aſh*, or *hepar ſulphuris*, *lime-water*, &c. Sometimes he allows ſome alterative means, according to the nature of the Goitre, of which he diſtinguiſhes *twelve* kinds. But before giving his powder he adminiſters one or two emetics, and ſome doſes of ſalts in the courſe of the treatment. He does not agree with Foderè concerning the influence of the moon, which he conſiders as quite ineffectual.

We have before us a Latin diſſertation of Dr. Valentin, on the ſame ſubject, printed at Nancy, in the year 1789, bearing this title—*De Struma Bronchocele Dicta et de Hemeralopia.* It is far leſs extenſive than the memoir he gave two years after; for, in the former, he divides the Goitre into *ten* ſpecies, of which he gives a particular account. It is

* We are appriſed, by a correſpondent, that the Medical Society of Paris, under the title of the *Society of Health*, intends ſoon to publiſh the ſequel of works ſent to it, and to the Academy of Surgery, both being now united into one.

remarkable, that in this piece too he diſapproves the uſe of *ſtrong* alkaline ſubſtances, *when ſwallowed*, on account of their bad effects, not only on the ſtomach, but alſo on the nervous ſyſtem, and the ill conſequences reſulting from their abuſe.

We conclude this article by expreſſing our hopes that Profeſſor Barton may hereafter continue to inveſtigate this ſubject with the ſame caution, warineſs and candour, as well as with the ſame comprehenſive and enlightened views, which ſo eminently diſtinguiſh his preſent Memoir.

ART. II. *Memoir on the Analyſis of the Black-vomit, ejected in the laſt Stage of the Yellow Fever. By* Iſaac Cathrall. *Philadelphia.* Folwell. 8vo. pp. 32. 1800.

OFTEN has it been our lot to remark, that there is no aſſignable proportion between the length or ſize of a publication and its merit or value. Readers of books have long lamented that a large amount of the time and toil they beſtowed in examining the publications of their day, was expended without either inſtruction or entertainment. Doleful is the taſk of the reviewer who is to give an account of long and tedious works, and to glean, from an almoſt immeaſurable ſurface of leaves and pages, the few particles of ſenſe and beauty which are ſcattered over them. Amplification and prolixity are the common faults of writers: and it is a great pity, both for them and their readers, that theſe are ſo prevalent. Fortunately, however, it happens, that they who offer their compoſitions to the public, do not always fill a folio or a quarto with that which might be comprehended in a pamphlet of moderate ſize. In ſeveral departments of learning, there are ſhort works which have ſecured extenſive and laſting fame to their authors. The celebrated treatiſes of LONGINUS *on the Sublime*, of HORACE *on the Art of Poetry*, and of TACITUS *on the Manners of the Germans*, are none of them remarkable for their length. LITTLETON's luminous tract *on Tenures*, the oracular book of Engliſh lawyers, runs but a little way in print. The admirable *Theory of Matter*, propoſed by BOSCOVICH, is contained in a volume of ſmall bulk. In ſhort, two of the moſt valuable phyſiological eſſays; to wit, the one *on the Concoction of Food in the Sto-*

mach, by STEVENS; and the other *on the Reſpiration of Animals*, by GOODWYN, compriſe their important experimental matter within a very moderate compaſs.

To this claſs of conciſe, but pointed and ſententious productions, belongs the memoir which now lies before us. Without any preface or circumlocution, Dr. Cathrall proceeds directly to his ſubject. To this he has attended patiently and perſeveringly. That the piece may be rightly appreciated, it is proper to obſerve, that the author began his obſervations during the rage of the fatal ſickneſs at Philadelphia, in 1793; and offered to the Philoſophical Society of that city, in the month of June, 1800, this reſult of his ſeven years inveſtigation: and that many of the experiments were witneſſed by Drs. Samuel Duffield and Adam Seybert. The black-vomit is deſcribed thus: (p. 4—7).

"The black matter, or vomit, ſo called, appears to be of two kinds. One conſiſting of a number of black flaky particles, reſembling the grounds of coffee; the other of a dark-coloured inſpiſſated mucus. Of each of theſe I ſhall give a ſeparate deſcription.

"This flaky diſcharge was always preceded by violent ſickneſs and vomiting; and, as a precurſor to the ejection of this matter, in ſome caſes, the patients vomited a fluid like whey or muddy water, or one conſiſting of a brown flaky ſubſtance, reſembling chocolate or ſpoiled porter, mixed with browniſh-coloured mucus.* Theſe ſubſtances were ſometimes of a lighter colour, and were ſuſpended in a glarey yellow-coloured fluid, which became nearly tranſparent when at reſt, by the ſubſiding of a ſmall number of brown particles. This coloured matter was generally vomited in ſmall quantities, and with conſiderable difficulty; but when the black flaky diſcharge commenced, it was frequently ejected in large quantities, and with ſimilar force to a fluid from the action of an emetic. As the diſeaſe advances, this matter aſſumes a darker colour, and its quantity ſometimes becomes ſo much augmented, that I have known one gallon vomited in 48 hours, beſides a conſiderable quantity, which was of a much thicker conſiſtence, that was diſcharged by the bowels. This black-vomit, after ſtanding ſome hours, depoſits a black flaky ſubſtance, from a

* "The chocolate, or coffee ſickneſs, or the black ſickneſs, ſays Dr. de Monchy, is not taken from the blackiſh hue or ſhade of the ſkin, but it is derived from the fœtid, blackiſh matter diſcharged from the firſt paſſages. *See Diſeaſes in Voyages to the Weſt-Indies.*"

glarey yellow-coloured fluid, ſimilar, in appearance, to an infuſion of green-tea. Theſe depoſitions were ſometimes in diſtinct particles, but frequently in a kind of dark powder. The above particles were various in ſize, and of a very irregular figure, not unfrequently mixed with pieces of the villous coat of the ſtomach. Theſe may be diſtinguiſhed by their being longer in ſubſiding to the bottom of the veſſel than the flaky ſubſtance. There were ſome diſproportions between the yellow-coloured fluid and the quantity of flaky ſubſtance, as in the other appearance of the vomit. The flaky matter was very readily re-incorporated with the yellow-coloured fluid, by the leaſt agitation of the veſſel; and when kept in a phial, corked for eight or ten days, aſſumed rather an agreeable, ſaccharine odour, and was extremely briſk, like fermenting beer. This laſt property is not peculiar to this fluid, but common to ſome other animal ſecretions. When the black-vomit was kept for two years in a ſtate of reſt, the flaky particles became perfectly ſeparated. On agitating the veſſel, the former was immediately incorporated with the latter; and, after remaining at reſt ſix months, ſhowed ſcarce any diſpoſition to ſeparate.

"The mucus matter which was ſometimes vomited in the yellow fever, and particularly in that which appeared in 1797, was very ropy, and of a black colour. This matter floated on a fluid of a dark colour, which appeared to receive its tinge from the colouring matter of the mucus. When this matter was agitated in a phial, the mucus ſhowed no diſpoſition to mix with the fluid part of the vomit, and when it was repeatedly waſhed in clear water, became nearly of the colour of the mucus ſecreted in the alimentary canal. This black matter was diſcharged, in large quantities, in the caſes which proved mortal in 1797, and was a very inactive fluid when applied to the moſt ſenſible parts of the healthy body, and was eſſentially different from the coffee-ground vomit."

From various and repeated experiments, Dr. Cathrall concludes that the black-vomit, beſides a conſiderable proportion of *water*, tinctured with *reſinous* and *mucilaginous* ſubſtances, contains a *predominant acid*, which is neither the carbonic, phoſphoric, nor ſulphuric. This *acidity* he found to be preſent in the *yellow-coloured* fluid, taken from twenty different patients, during ſeveral ſeaſons of the prevailing yellow fever, and alſo in the *black flaky* ſubſtance. With theſe were combined muriate of ſoda, iron, and an unctuous animal ſubſtance, ſomewhat reſembling ſpermaceti. The exact proportion of

the different ſubſtances he had not an opportunity of inveſtigating, for want of a ſufficient quantity of the black flaky matter to make a complete analyſis. An *acid*, which he believes to be of the ſame quality, is contained in the fluids ejected from the ſtomach a few hours *before the commencement* of black vomiting. Of this acid the author has expreſſed himſelf in *negative* rather than *poſitive* terms, faintly hinting, however, that it may be the *muriatic* (p. 9).

On the effects which the matter of black-vomit produces on the living ſyſtem, Dr. C.'s experiments are ſo original, and his concluſions ſo remarkable, that we ſhall inſert the ſection at full length: (p. 19—23).

"From the internal ſurface of the ſtomach and inteſtinal canal appearing, on diſſection, inflamed and ſphacelated, particularly in ſome patients who had vomited black, it has been believed that the black-vomit was corroſive, and had a power of acting on parts it came in contact with.* This power has likewiſe been inferred from ſome patients complaining of a ſoreneſs in their throats, immediately after the ejection of this black matter.

"To determine how far it was capable of acting on the healthy body, it was ſubmitted to the following experiments:

"1ſt. In October, 1794, immediately after a quantity of black-vomit was taken out of the ſtomach, after death, I applied ſome of it to my tongue and lips: to the latter it gave, a ſhort time after application, the ſenſation of a fluid perceptibly acrid. This experiment was, the next day, ſeveral times repeated, with the ſame reſult.

"2d. A friend of mine applied it to his lips, and it produced a ſimilar ſenſation, but would not affect his tongue.

"3d. Finding the effects of this matter ſo different from what was expected, I began to believe that this diſcharge varied materially, in point of activity, in different patients; but on ſubjecting the black-vomit, procured from a number of perſons, to the ſame teſt, it produced the ſame effect.

"4th. Two ounces of a fluid, reſembling chocolate, was obtained, which was vomited a few hours before death. This was applied in the ſame manner; but there could not be perceived any difference in the reſult.

"5th. In the beginning of October, 1799, Mr. Joſeph Parker, an active and intrepid member of the Board of Health, obligingly preſented me with five ounces of black-vomit, ob-

* "See Deſportes on Diſeaſes of St. Domingo, p. 203, vol. i."

tained from the phyſicians of the City Hoſpital. Some of this I applied to my tongue, in his preſence, but could not perceive the leaſt corroſive effect. When this fluid was applied to the ſkin, on different parts of the body, it produced no other effect than what water did of the ſame temperature. I have often immerſed my hand in black-vomit, immediately after it was diſcharged from the ſtomach, and whilſt it was warm, without exciting the leaſt uneaſy ſenſation in the ſkin.

"*(a)* October 4th, 1799, three cats were confined in a room, and fed with beef, which had a conſiderable quantity of the flaky ſubſtance of the vomit inſerted into it. This manner of feeding was continued until they had ate one drachm and an half of the flaky ſubſtance, and had drank ſeveral ounces of the black-vomit. On the 5th, the excretions by the bowels were of a dark colour; yet there could not be diſcovered any difference in their health; but, from their being ſtangers to each other, they had a conſtant propenſity to combat. This malicious ſpirit continued until the 20th, when they were diſmiſſed in good health.

"*(b)* A large dog was confined in a room, and, by an aſſiſtant, his jaws were forced aſunder, and he was compelled to ſwallow an half-pint of black-vomit. The following day the excretions by the bowels were fluid, and of a black colour; but there could not be obſerved the leaſt alteration in his health, from the time of making the experiment until he was diſmiſſed, which was about three weeks after.

"*(c)* Two full grown fowls were confined, and fed with bread, ſteeped in black-vomit, for twelve days. This, Mr. Parker, as well as myſelf, obſerved, they ate with great avidity; but it had no evident bad effect upon their health; for they continued as well after as they were before the experiment, and ſeemed to give the preference to that kind of food to every other which was preſented to them, and they appeared to thrive equally as well as if they had been fed upon corn.

"*(d)* On the 3d of October, 1799, in a ſmall yard, adjoining the houſe in which I live, ſeveral ounces of the black-vomit, recently obtained, was [*were*] evaporated over a moderate heat, in order to obtain the flaky ſubſtance. During this experiment, Mr. Parker held his head over the veſſel for ſome minutes, ſo as to inhale the ſteam of black-vomit; after which we continued within two yards of the veſſel, without experiencing any unpleaſant effect.

"*(e)* The following day I cauſed the windows and doors of a room to be cloſed, and the ſame experiment was repeated

on a ſand-bath, conſtructed in the middle of a room. The fluid was evaporated until the atmoſphere was ſo impregnated with the effluvia of the vomit as to render the apartment extremely unpleaſant, not only from the odour of the vomit, but the warmth of the room. In this atmoſphere I remained one hour, during which I had a conſtant propenſity to cough, and had, at times, naufea and inclination to vomit; but, after walking out in the air, theſe effects gradually ſubſided. I experienced, however, a ſenſe of wearineſs at my cheſt for many hours after.

"From the above experiments, it appears that the black-vomit, when applied to the moſt ſenſible parts of the body, produced little or no effect.

"Secondly. It appears that large quantities of this fluid may paſs through the ſtomach and bowels of quadrupeds and other animals, without apparently diſturbing digeſtion, or affecting their health. This fact inconteſtibly proves the inactivity of this fluid, and renders it probable, that the ſpeedy death which enſues, after this diſcharge in yellow fever, is not from the deſtructive effects of this matter on the ſtomach and bowels, but, moſt likely, from the great degree of direct or indirect debility which had been previouſly induced, on which the black-vomit is ſometimes an attendant, and ſtrongly expreſſes the great danger to be apprehended from the enervated ſtate of the ſyſtem.

"Laſtly. The experiments *(d* and *e)* tend, in ſome meaſure, to prove, that an atmoſphere, highly impregnated with the odour of black-vomit, recently obtained, would not produce fever, apparently under the moſt favourable circumſtances."

Dr. C. next examines the opinions of authors concerning the black-vomit. Theſe he claſſes under four heads. 1. That which conſiders it as conſiſting of putrid bile: 2. As compoſed of a mixture of blood and bile: 3. Of the villous coat of the ſtomach diſſolved in the progreſs of inflammation, terminating in ſphacelus: and, 4. Of bile mingled with the *nitric* (we ſuppoſe he means *ſeptic)* acid contained in the alimentary canal. All theſe are conſidered and diſmiſſed as inadequate or erroneous, and are ſucceeded by the author's own opinion, that black-vomit is *an altered ſecretion from the liver.* Dr. C. grounds this conviction on the following conſiderations: (p. 29—32).

"The colouring matter of the vomit appears, from the authors already quoted, to be generally traced, after death,

to the gall-bladder. This poſition being incontrovertibly eſtabliſhed by diſſections, the power of the liver to ſecrete that ſubſtance will be admitted, of courſe, as it could not be ſecreted by the gall-bladder, or tranſmitted into that viſcus through any other paſſage, but by the hepatic duct. If this view of the ſubject be, in any meaſure, juſt, it is a fact, aſcertained beyond the ſhadow of a doubt, that the black flaky ſubſtance of the vomit is an altered ſecretion from the liver. This matter, being ſecreted by the liver, and depoſited by the hepatic duct in the gall-bladder, in the laſt hours of this diſeaſe, is from thence forced, by the contractions of the gall-bladder and cyſtic duct, in conjunction with the violent action of vomiting, into the ſtomach. It there receives the addition of the yellow-coloured fluid, which is almoſt always ejected with the flaky ſubſtance. That this fluid is combined with the flaky matter in the ſtomach, and not in the gall-bladder, every inquiry into the appearances after death fully confirms. This circumſtance renders the yellow-coloured fluid ſubject to ſome difference in its properties, according to the nature of the fluids received into the ſtomach a ſhort time before vomiting; but all that I have had an opportunity of examining, have nearly the appearance we have already deſcribed. That the ſecretory economy of the liver may be ſo far arreſted in its healthy action, by the progreſs of diſeaſe, as to aſſimilate a fluid having not the leaſt analogy to bile, every work on morbid diſſections certainly prove [*proves.*] Lieutaud mentions a caſe from Rivalerius, in conſequence of a diſeaſed liver, where the fluid in the gall-bladder reſembled milk; and Storke relates a caſe of a dropſy ſucceeding an intermitting fever, where the fluid in the gall-bladder reſembled the white of an egg. To theſe I may add one that came under my own obſervation, of a gentleman who died dropſical, in conſequence of an enlarged liver. The gall-bladder contained a fluid, of a dark colour, having not the leaſt reſemblance to bile. Theſe, and many more caſes, could be adduced to prove the power of the liver, under certain circumſtances, to ſecrete a fluid diſſimilar to bile; but it would be needleſs to recite them, as the inſtances already quoted are, no doubt, ſufficient to eſtabliſh the fact. This peculiar condition of the ſecretory veſſels, in the yellow fever, is not confined ſolely to the liver; for we find that other ſecretory functions are ſometimes affected in a ſimilar manner, during the ſame diſeaſe, and nearly at the ſame period of time. In confirmation of theſe obſervations, I believe moſt phyſicians muſt

have remarked, that, in ſome caſes, the kidneys, during the period of black-vomiting, ſecrete a fluid of a dark colour, which has a thick pellicle on its ſurface, and appears almoſt as different from urine as the black-vomit does from bile. This diſcharge is generally a precurſor to a ſymptom which never fails to predict a ſpeedy diſſolution, viz. a paralyſis of the ſecretory functions of the kidneys.

"The more I conſider the material change produced in the different ſecreting veſſels, during the laſt ſtage of this diſeaſe, the more this theory appears to be ſupported by reaſon and the plauſibility of truth. But, though a morbid condition of the glandular economy of the liver may produce the coffee-ground coloured vomit, it does not ſeem probable that the black inſpiſſated mucous matter which was ejected in the caſes that proved mortal in 1797, is derived from the ſame ſource; for the liver, under no condition of diſeaſed action that we are acquainted with, is capable of ſecreting mucus of ſuch an appearance; therefore, we think it moſt reaſonable to refer it to the ſurfaces, which are deſtined, in a ſtate of health, to ſecrete mucus. Now, admitting the axiom, 'that ſimilar cauſes produce ſimilar effects, under ſimilar circumſtances,' why may not the glandular ſtructure of the ſtomach be affected in a ſimilar manner to that of the liver and kidneys, ſo as to enable it to ſecrete the mucous matter above-mentioned? This opinion, I think, may be affirmed by other analogies, not only in the ſthenic, but in the aſthenic condition of ſecreting ſurfaces, in which there are equally as great a deviation from healthy ſecretion as the one alluded to. This we have clearly exemplified in veſſels deſtined to ſecrete mucus in a ſtate of health; but, when labouring under inflammation, evidently ſecrete pus."

The obſervations with which we ſhall conclude our review of this excellent piece will not be very long. Firſt: It appears clearly that the black-vomit is not the *cauſe* of yellow fever, but merely a ſymptom, or rather an *effect*, of the poiſon originally applied. It is poſſible, that not a drop or particle of the ſeptic virus itſelf, which induced the diſeaſe, is preſent in the *primæ viæ* when black-vomiting begins. It may have been wholly ejected by previous vomitive efforts, and the irritation of inflammation, cauſed by its preſence, ſtill remain to torture or to kill the ſufferer. This may be underſtood by compariſon with another poiſon; arſenic, for inſtance. If the oxyd of arſenic ſhould be ſwallowed, it is poſſible for every atom of it to be expelled by vomiting; but, even then,

the injury done by it to the ſtomach would remain, and a dangerous gaſtritis, followed by black-vomiting, might deſtroy life. And neither in this caſe of *arſenical fever*, nor in the *ſeptic* or *yellow fever*, would the matter vomited in the latter ſtages of the diſeaſe neceſſarily contain any oxyd of the metal, or acid of putrefaction. Secondly: The black-vomit, as has been ſhown by Dr. Miller, in his curſory obſervations on yellow fever, publiſhed in Med. Rep. vol. ii. p. 412, is by no means a *pathognomonic* ſign of that diſeaſe. So far is it from being the caſe, that this awful ſymptom frequently follows the ſwallowing of the oxyd of arſenic, muriate of quick-ſilver, and the acetate of copper. It is alſo a frequent conſequence of the fatal operation of vegetable poiſons taken into the ſtomach. They, therefore, who have conſidered it as a *diagnoſtic* of the yellow fever, have judged erroneouſly, and drawn their concluſions from very partial and limited premiſes: for, inſtead of being confined to this diſtemper, it is now well known to attend or follow the ſevere and deadly operation of moſt poiſons upon the organ of digeſtion. Thirdly: Though we are inclined to think, with the author, that former writers have given but conjectural and imperfect accounts of this dark-coloured fluid, and that he has reaſoned ably on the ſubject, yet we ſhall offer a few ideas on the function of the liver in theſe caſes. This conglomerate gland, during the time of health, prepares a *bitter* and *alkaline* liquid in conſiderable quantity. The uſe or operation of this, as far as we can interpret final cauſes, is to quell and neutralize acids of all kinds in the chyle or alimentary maſs, as it paſſes through the inteſtinal canal. And a wiſe and happy proviſion it is of the Creator, that the animal economy is furniſhed with an ample ſtore of ſuch an antiſeptic and antipeſtilential liquid. But the ſecretory proceſs of the liver, as well as of the other glands of the body, is ſometimes impaired. Thoſe *violent* poiſons, which paralyze the lachrymal gland, dry up the tears, benumb the ſalivary glands, prevent the formation of ſpittle, torpify the kidneys and diminiſh the quantity of urine, and, affecting the cutaneous veſſels with ſpaſm, check the inſenſible perſpiration, ſeem alſo to interrupt the bile-producing function. The ſupplies of wholeſome fluid, which the liver was accuſtomed to prepare, are withheld, and the inteſtines, at length, contain the common *ingeſta* and *excreta*, with all their proneneſs to become ſour and to irritate, without the ſoothing and controuling influence of the gall. The predominant acidity, in Dr. C.'s experiments, may *thus*

be accounted for, and, at the ſame time, ſerve to render it yet doubtful whether the matter of black-vomit proceeds from the liver. We have no proof that the liver ſecretes an *acid* liquor. During the prevalence of that ſymptom, we are inclined to believe the ſickly liver prepares too *little* bile for the wants of the conſtitution, and this *little* may be altered ſo in its quality as, on meeting with the acidity of the ſtomach and duodenum, to exhibit a dark colour, in a manner not materially different from that quoted (p. 24) as Profeſſor Mitchill's.

ART. III. *Recherches ſur la Medecine, ou l'Application de la Chimie a la Medecine. Par* François Blanchet. *A New-York.* Pariſot. 8vo. pp. 246. 1800.

WITHIN the laſt half century the empire of chemiſtry has been ſurpriſingly enlarged. From a limited authority over *metals* and *medicines*, ſhe has extended her ſway to the various kinds of *earths* which enter into the compoſition of our planet, to the *waters* which flow on its ſurface, and the *gaſes* which conſtitute its atmoſphere. Not contented with ſublunary power, ſhe claims a dominion as extenſive as the ſolar influence, and gives law to *heat*, *light*, and *fire*, whereſoever they exiſt in the univerſe. This, one would think, was enough; but all this does not content her. With reſtleſs activity, ſhe breaks down organic matter after death, reſolves it into its priſtine elements, or into new compounds, and, according to the temperature of the ſeaſons and the quality of the materials, fertilizes the ſoil with manure, or contaminates the air with poiſon. And here her daring enterpriſe would ſtop; but ſome of the leaders moſt devoted to her ſervice, have flattered her that ſhe was miſtreſs of the *animated* as well as of the *inanimate* world. "Life," they ſaid, "was in her gift. By her direction, heterogeneous atoms could arrange themſelves into filaments, and filaments bend to circles; and, as filament placed itſelf beſide filament, and circle aſſociated itſelf to circle, muſcles and veſſels, by an eaſy proceſs, were conſtructed. To theſe, by a neceſſary conſequence, *irritability*, or a *ſuſceptibility* of *impreſſion*, inhered. If, proceeding from this *ſimple* form, ſhe choſe to be a little more *complicated*, ſhe could combine other atoms into brain and nerves, and impart to them *ſenſitive* and *voluntary* power: and, by modifying theſe again into *locomotive*, *digeſtive*, and

generative organs, creatures, of various degrees of vital energy, could be produced. For theſe it was only neceſſary to aſſociate iron, carbon, and a few other things, to water, in order to form *blood*—and to connect phoſphoric acid with lime, to produce *bone*—and ſuddenly their hearts would throb within their breaſts, and their weights be ſupported on their limbs. Laſtly, out of this combination of elements, *mind* would ariſe as one of their modifications, and intellect, memory and paſſion depend thereon for their exiſtence."

But theſe ſentiments are too enthuſiaſtic. The proper province of chemiſtry is the inorganic or lifeleſs part of creation; or, if it is applied to the explanation of vital phenomena, it ſeems to ſucceed beſt in thoſe parts of the conſtitution which poſſeſs irritability and ſenſibility in the loweſt degrees. That exquiſite organization, on which pleaſure and pain, hope and fear, calculation and invention depend, has hitherto defied the laws of chemiſtry, and refuſed ſubmiſſion to her power. There is, however, a medium between the two extremes, of thoſe who aſcribe *every thing*, and ſuch as attribute *nothing*, in the vital economy, to chemical agency. DARWIN has endeavoured to purſue this courſe. In his Zoonomia he has employed chemical reaſoning as far as it appears to him applicable and proper; and, beyond the reach of this ſpecies of philoſophy, he has attempted an inveſtigation of the *laws of organic life*. The extravagance of Girtanner, and ſome others, ought to be carefully avoided.

The author of the treatiſe now under conſideration, ardent in the purſuit of ſcience, and zealous to interpret the phenomena of animated nature, has undertaken to explore ſome of the darker receſſes, with the torch of chemiſtry in his hand. In the courſe of his reſearches, which are comprehended in twelve chapters, he treats concerning the effects of oxygen and caloric on the animal body; of the inſenſible perſpiration; of the conſtitution of acids; of the operation of poiſons; of electricity; of the effects of cold; of the phyſical cauſe of menſtruation; of ſleep; of cathartics; of emetics; of the influence of comets and volcanos upon the atmoſphere; and of light: to which is added, a letter, of nearly thirty pages, from Mr. Blanchet to one of his friends in Quebec, on the yellow fever. In a preliminary diſcourſe are contained ſome obſervations on ſyſtems of medicine, and on chemical nomenclature.

But to deſcend a little into detail.—Mr. B. dwells very particularly on the effects of caloric and oxygen, the two agents which, in his opinion, produce moſt remarkable effects on

the living body, tending, by different modes of operation, both to promote its decay and to keep it alive. For the information of our readers we have tranſlated the paſſages:

"We conclude this ſection (p. 17) by affirming that our old age, and the death which terminates it, are occaſioned by oxygen and caloric operating on us during a ſeries of years; and, when this admirable fabric of our bodies crumbles down, from the unceaſing effect of theſe two powerful principles, we return to the ſource whence our exiſtence was derived. In vain ſhould we ſtrive to withdraw ourſelves from the law which demands of us this tribute."

"We conclude this chapter (p. 27) by giving it as our opinion, that, though vital air tends to deſtroy the animal conſtitution inceſſantly, and without remorſe, yet life is the effect of the operation of this air jointly with caloric, in an organized body; and although, in man and other animals, the brain is the part in which all the ſenſes are concentered, by means of the nerves, which ſeem to poſſeſs an energy of their own, ſtill we believe they derive their influence from the common cauſes, vital air and caloric, as all the other organs of the body do. It is idle to ſuppoſe the nerves contain a peculiar fluid, which fills them, and gives them motion. It might be ſaid the muſcular fibres were quite as much in need of a fluid to enable them to act, contract, dilate and move."

"Every thing concurs to prove," ſays Mr. B. (p. 15), "that oxygen, aided by caloric, combines with the human body, and tends to deſtroy it every moment while it laſts."—And then again, in p. 25, he expreſſes himſelf thus: "To form the univerſe, Des Cartes aſked for nothing more than matter and motion. To give life, I merely want an organized machine, oxygen and caloric."

All theſe effects, he thinks, are brought about by the chemical combination of theſe two ſubſtances, into oxyds and acids, with the different baſes which the animal conſtitution contains; forming ſweat, urine, ſperm, and other ſecreted fluids. The accumulation of caloric and oxygen in the ſyſtem, in conſequence of obſtructed excretions, gives riſe to both general and local inflammation.

Purſuant to theſe ideas, Mr. B. in his chapter on acids, contends that they are compoſed not merely of radicals united to oxygen, but contain a very large quantity of fixed caloric. This, our readers will recollect, is contrary to the common doctrine; oxygenous gas loſing its caloric on combining with an acidifiable baſis into an acid. Acids, he thinks, are active,

becauſe they poſſeſs caloric and oxygen, and are violent in proportion to the quantity they contain, and the eaſe with which the two elements break looſe or are ſeparated. To illuſtrate the author's meaning, we ſhall take one of his own examples. Though Mr. B. expreſſes his perfect conviction that ſeptic acid, or its gas, according to MITCHILL's doctrine, is the exciting cauſe of peſtilential diſtempers (p. 75), yet he conſiders the azotic baſis of this acid as having very little to do in the buſineſs. It is not ſo much the *tertium quid*, formed by ſepton and the principle of acidity, which cauſes the miſchief, as the caloric and oxygen, extricated ſuddenly and copiouſly from that wonderful acid, on its decompoſition in the animal conſtitution: and all other acids he conſiders active for a like reaſon. Notwithſtanding the ingenuity of Mr. B. we ſtill are of opinion, that *all* the ingredients of an acid, its *radical* as well as its caloric and oxygen, conſpire to make it what it is, and are of great moment in its conſtitution.

This diſcuſſion leads Mr. B. to give a theory of poiſons (p. 79); and from a review of the effects of cantharides, the venom of the viper and other ſerpents, opium, ſtramonium, cicuta, digitalis, arſenic, lime and ſtrong liquors, he concludes that they owe their activity to the liberation of much impriſoned fire, condenſed within them. He judges in the ſame manner of the virus of the rabid dog, of ſyphilis, and of the ſmall-pox. We ſhall not deny that all theſe things may be ſo; but we own our conviction would have been ſtronger, if, in the ſprightly and rapid career of the author, he could have ſtopped to ſtate more pointedly his proofs.

Mr. B. aſcribes death by lightning to an immediate evolution of caloric in the conſtitution, forming inſtantaneouſly, among other new compounds, *in articulo mortis*, a *portion of ſeptic acid* (p. 116).—In p. 151 & ſeq. he comments upon the opinions of BROWN, DARWIN, and ſeveral other reſpectable writers; and here, as in the other parts of his work, he exerciſes his critical powers with boldneſs and freedom. The ſeventh chapter contains his ſentiments on the menſtrual flux, which, he ſays, "is undoubtedly cauſed by a ſuperabundance of caloric and oxygen accumulated in the blood, and exerting their influence particularly on the womb." (P. 156).

But in a work which treats of ſuch a variety of important matter as the one before us, it is difficult, in a review, to do juſtice to *all* its parts. In the diſplay we have given, our readers will have perceived numerous indications of a mind

vigorous and glowing, diſengaging itſelf from the influence and authority of others, and reſolutely determining to think for itſelf. It is the happineſs of civilized America, that its inhabitants are leſs trammelled by ancient uſage and prejudice than the people of any other part of the globe. Mr. B. has exerciſed the privilege of an American, and ſpeculated on the *medicina chemica* in a manner which, in many reſpects, we think peculiar to himſelf. We hope the French language, in which he has written his book, will be no objection to its being read in the United States. In Canada, the author's native province, we ſuppoſe it will have an extenſive circulation; and, wherever it is peruſed, give a ſpecimen of the learning, taſte and talents of the author. From his induſtry and ability, we preſage a large ſhare of uſeful exertion in the future part of his life.

ART. IV. *A Proſpect of exterminating the Small-Pox: being the Hiſtory of the Variolæ Vaccinæ, or Kine-Pox, commonly called the Cow-Pox, as it has appeared in England; with an Account of a Series of Inoculations performed for the Kine-Pox in Maſſachuſetts. By* Benjamin Waterhouſe, *M. D. Fellow of the American Philoſophical Society, Academy of Arts and Sciences, &c. &c. Cambridge.* Hilliard. 8vo. pp. 40. 1800.

OUR readers will recollect, that in the ſecond volume of the Medical Repoſitory, p. 255, we gave them ſuch an account of the cauſes and effects of the variolæ vaccinæ as we had received in that part of the year 1798. In our vol. iii. p. 70, we gave a further account of the proceedings with this ſingular poiſon in Great-Britain. In the ſame volume, p. 310, it was announced, that Dr. Miller had received ſome of the virus, very obligingly forwarded to him by Dr. George Pearſon, of London. A ſummary of the experience by inoculating with it in Europe, was given in p. 315. And in our preſent volume, p. 88, we inſerted further information on the advantages to be derived from the ſubſtitution of this new contagious diſeaſe in the place of the ſmall-pox.

Since the ſpreading of the ſmall-pox over the earth, by the increaſed intercourſe among mankind, its violence has been ſeverely felt by moſt civilized nations, and frequently by the

uncivilized people who have traded with them. Dreadful were its ravages in Europe, before the Aſiatic method of leſſening its fury by inoculation was introduced. And even ſince that time, and the adoption of this artificial and ſafe method of infecting the conſtitution with the venom, its benefits have been but partially experienced: for, in ſome communities, the introduction of it, even in the inoculated form, has been prohibited by laws; and, in others, where no ſuch prohibition exiſts, the expenſe and trouble of undergoing the diſeaſe have debarred many from ſubmitting to its operation.

It has ſeemed politic and proper to the governments of thoſe parts of federated America, called the New-England States, to prevent, by various legiſlative proviſions, the introduction and ſpread of this formidable diſtemper among their citizens, in the natural way, and, in ſome, by inoculation. Of theſe regulations, a good and inſtructive account has heretofore been drawn up by the learned author of the piece now before us. We know not of any ſimilar laws in any other part of the United States. In conſequence of ſuch ſtatutes, it has many years been cuſtomary for perſons in New-England, who wiſhed to have the ſmall-pox, to come to New-York for the purpoſe of being inoculated, and, after their recovery, to return home. There was an inconvenience in this, but this was thought a trifle compared with the evil of its indiſcriminate introduction. In ſea-port towns, poſſeſſing a large ſhare of foreign commerce, it has been found impoſſible to exclude this malady altogether. In ſpite of all poſſible precautions, the contagion would, at certain times, be ſecretly introduced. To relieve themſelves, in ſome degree, from the perpetual anxiety of having the ſmall-pox ſpread among them in the *natural* way, the inhabitants of the town of Boſton, a few years ago, underwent a general inoculation, by common conſent.

Notwithſtanding the mitigation of the ſymptoms excited by the variolous poiſon, when introduced by art into a body duly prepared for its reception, benevolent men, in different countries, have formed projects for a total extinguiſhment of the caſual kind. Some of theſe philanthropiſts had, in the warmth of their zeal, perſuaded themſelves that ſuch a plan might be carried into effect, and ſhown how the multiplication of this terrible poiſon might be ſtopped. But while theſe friends of mankind, in various parts, were maturing their ſchemes, and labouring to bring them into practice, a diſcovery was made by an Engliſh phyſician, which is likely to ſupercede them all. This conſiſts in the facts aſcertained by Jenner, that a new

ſpecies of virus, called "vaccine matter," does, by inſertion in a bleeding wound, ſo affect the human conſtitution as to render it unſuſceptible of the action of "variolous matter;" and that, in working this deſirable change, it ſcarcely puts either beauty or life in jeopardy. It does not ſpread from perſon to perſon through the medium of common air.

To a people ſo circumſtanced as our fellow-citizens in New-England are, it would ſeem that ſuch a diſcovery would be deemed peculiarly important. They will thereby be enabled to fortify their conſtitutions againſt the ſmall-pox, by ſubmitting to inoculation for a diſtemper by far more lenient, and which appears to be contagious *by contact alone*, and not by diſperſion through the atmoſphere. Information, however, muſt be gained of the diſcovery, hiſtory, progreſs and effects of this extraordinary contagion, before men will conſent to be infected by it. To give the public ſuch information, is an object of Dr. W. in the preſent piece.

We need not inform our readers that the author is the Profeſſor of the Practice of Phyſic in one of the moſt reſpectable of the American univerſities. A publication of this kind comes with peculiar force from ſuch a character; and, we doubt not, will attract a good deal of notice. Profeſſor W.'s pamphlet is divided into three chapters; to which are added an appendix and a poſtſcript. The firſt is chiefly employed in relating, that after receiving, in 1799, Jenner's, Pearſon's and Woodville's publications on the cow-pox, he made known ſomewhat of their contents to the inhabitants of Boſton, through the newſpapers, in the months of March and November of that year, and mentioned the ſubject at a meeting of the American Academy of Arts and Sciences, in one of the rooms of the univerſity. In his ſecond chapter, Dr. W. relates how, after ſeveral diſappointments, he at length procured ſome of the vaccine matter from England, and, with it, inoculated all the younger part of his own family, and ſeveral other perſons. The virus produced a diſeaſe ſimilar to that deſcribed by the Engliſh phyſicians. He informs, in the third chapter, that ſome members of his family, who had ſuffered the vaccine malady, were afterwards inoculated at the ſmall-pox hoſpital, in the neighbourhood of Boſton, by Dr. Aſpinwall, and that his ſon, who had been the ſubject of the firſt experiment, was found to be proof againſt the ſmall-pox poiſon. The other five were under trial at the time of publication. We have ſince underſtood neither of them ſickened with the variolous contagion. He concludes by ſtating various

good and ſolid reaſons in favour of the vaccine inoculation, and in explanation and removal of objections brought againſt it.

We had read the contents of the *appendix*, before we ſaw it here, in the newſpapers; and in the *poſtſcript* we obſerve a valuable practical caution to the people, "leſt they conceive too lightly" of this diſeaſe, which, "mild and ſafe as it is, requires more of the phyſician than merely putting the matter into the arm."

It ſeems, at firſt glimpſe, a little odd, to congratulate the inhabitants of the weſtern hemiſphere on the introduction of a new diſeaſe among them; yet when a mild diſorder is ſubſtituted for one ſo wide-ſpreading and ferocious as the ſmall-pox, it muſt be owned there are few things of a medical nature which afford a better ſubject for rejoicing. It muſt be borne in mind, that the diſeaſe introduced is to expel one already among us; and in this deſirable work Profeſſor W. is entitled to the merit of having exerted himſelf among the foremoſt.

ART. V. *Tranſactions of the American Philoſophical Society, &c. Vol.* iv. *&c.*

[Continued from page 68.]

Experiments on Evaporation, by C. Wiſtar, M. D. &c.

THE object of this paper is to prove that evaporation may be produced, without regarding the degree of abſolute temperature, by merely creating a degree of temperature relatively lower than that of the evaporating ſubſtance, in the ſurrounding medium; or, in other words, that a ſlow diſtillation may be performed, with the common apparatus, by applying cold to the receiver or refrigeratory, without increaſing the heat of the retort or ſubſtance to be diſtilled, as there will be a continual paſſage of heat from the body, to be evaporated or diſtilled, into the air of the receiver.

With theſe views, Dr. Wiſtar poured an ounce and a half of vitriolic (ſulphuric) æther into a retort, and luted it to a receiver with a long neck, which was placed in a mixture of ſalt and ſnow, while the retort was ſurrounded by air of the temperature of 50° of Fahrenheit. The frigorific mixture, from the impurity of the ſalt, was ſeldom below 10°; ſo

that the difference between the æther in the retort, and the air in the receiver, did not exceed 40°. When the apparatus had been thirty hours in this ſituation, the frigorific mixture was removed, and one third of the æther was found diſtilled into the receiver. In order to be certain that the application of cold to the receiver really produced the diſtillation, Dr. Wiſtar prepared a ſimilar diſtilling apparatus, in the ſame manner preciſely, and placed the retort in contact with that of the other apparatus, while the receiver, inſtead of being chilled by the cold mixture, ſtood in air of the ſame temperature with the retorts, viz. 50°; but no diſtillation took place during thirty hours. To vary the experiment, ſome camphor was placed in another apparatus prepared as above, and the receiver was fixed in the frigorific mixture, while the retort ſtood in air of the temperature of 50°; at the expiration of thirty hours ſome of the camphor was found ſublimed, and the ſublimate had thoſe arboreſcent appearances which uſually attend when produced by heat.

Theſe experiments are ingeniouſly deviſed, and ſeem to have been conducted with warineſs and accuracy. The reſult is not only intereſting in a ſcientific point of view, but appears to open the way to practical improvements, and particularly to economy of fuel and time in managing the buſineſs of diſtillation.

Might not the uſe of iced water, or a ſtream of cold ſpring-water, running through the refrigeratory, upon this principle, facilitate the proceſs of diſtillation? When this buſineſs is conducted during the winter of our climate, ice may be obtained without much trouble or expenſe; and, in many ſituations, a ſtream of cold ſpring-water, fully adequate to this purpoſe, is always at hand.

A Memoir concerning the Faſcinating Quality which has been aſcribed to the Rattle-Snake, and other American Serpents, by Benjamin Smith Barton, M. D.

For an account of this Memoir, ſee our volume i. p. 79, firſt edition.

Some Account of an American Species of Dipus or Jerboa, by Benjamin Smith Barton, M. D.

This is a brief and impreſſive deſcription of one of the native quadrupeds of North-America. It is accompanied by a good plate. The lovers of zoology cannot fail to be gratified

as well by the deſcription itſelf as by ſeveral general obſevrations concerning that department of natural hiſtory, which are interſperſed with the account of this animal.

An Inquiry into the Cauſes of the Inſalubrity of flat and marſhy Situations; and Directions for preventing or correcting the Effects thereof, by William Currie.

In this inquiry Dr. Currie attempts to deliver ſome account of the compoſition of the ſoil of marſhes, which, beſides different earthy matters, he ſtates to conſiſt of animal and vegetable ſubſtances broken down by putrefaction, of carbon and nitre, and that this maſs, by diſtillation, affords oil, hydrogen and azote.

Finding that carbonic acid gas, hydrogen gas, and ammoniacal gas, are exhaled from the ſoil of marſhes, Dr. C. inquires whether any of theſe gaſes, in a ſeparate or combined ſtate, can be ſuppoſed to conſtitute the miaſmata ſaid to iſſue from ſuch low grounds. If carbonic acid gas, diluted with atmoſpheric air ſo as to become reſpirable, were to produce morbid effects, he thinks ſuch effects would be more likely to appear in the form of paralytic or comatoſe diſeaſes than in that of intermittent or remittent fevers. He rejects the opinion of the febrile influence of hydrogen gas itſelf, or of any combination of carbon and hydrogen in the form of gas; and forms, likewiſe, a ſimilar concluſion concerning ammoniacal gas.

Having thus decided upon the morbid qualities of the abovementioned gaſes, Dr. C. proceeds to deliver his own opinion concerning the cauſe of the inſalubrity of flat and marſhy ſituations as follows:

"From the facts and obſervations which have now been ſtated, I think it may be fairly concluded, that the cauſe of the unwholeſomeneſs of low and moiſt ſituations, in the ſummer and autumnal months, is not owing to any inviſible miaſmata or noxious effluvia, which iſſue from the ſoil and lurk in the air, but to a very different cauſe, viz. to a deficiency of the oxygenous portion of the atmoſphere in ſuch ſituations, in conſequence of vegetable and animal putrefaction, in conjunction with the exhauſting and debilitating heat of the days, and the ſedative power of the cold and damp air of the nights.

"For want of the refreſhing and ſalutary ſtimulus of pure air, all the functions of the body are performed imperfectly and languidly. The nervous ſyſtem in particular becomes preternaturally ſuſceptible of impreſſions from every change

that occurs in the temperature of the ſurrounding atmoſphere. The application of or expoſure to a damper and colder ſtate of the air than uſual, renders the veſſels on the ſurface of the body powerleſs and atonic, the brain and heart ſympathize with the extreme nerves and veſſels, the power of every function of the body declines, till the heart, rouſed by accumulating blood, re-acts with increaſing velocity, and is relieved of the unuſual burthen.

"That the cauſes which I have now aſſigned are the true ones, is rendered next to certain from the frequent occurrence of thoſe diſeaſes (which have heretofore been ſuppoſed to depend upon the operation of ſpecific miaſmata), in ſituations remote from marſhy ground, particularly in large and populous cities, where ſedentary occupations, and want of exerciſe, render the inhabitants delicate and infirm. I have ſeen numerous inſtances of this kind, even in the winter ſeaſon, when no effluvia from marſhes could poſſibly exiſt, eſpecially among thoſe who had been previouſly debilitated by other diſorders. Nor is it uncommon for perſons who have recovered from intermittents in the autumn, to have frequent recurrences of the ſame diſeaſe in the winter, merely from ſitting in a damp room, or other expoſure to cold."

The hypotheſis which aſcribes the prevalence of intermittent and remittent fevers, in marſhy ſituations, chiefly to a deficiency of oxygen, is now ſo generally abandoned, that it may be deemed ſuperfluous to undertake its refutation. With all the fallacy of eudiometrical experiments, made in the moſt careful manner, it appears that ſuch trials of the air of marſhes as have been moſt accurately performed with that inſtrument, are far from warranting ſuch a concluſion. But even admitting that the air of marſhes is deficient in oxygen, why ſhould we apprehend more miſchief from the abſtraction of oxygen, by means of putrefaction on that kind of ſoil, than by means of combuſtion, fermentation, or the reſpiration of animals? If merely the ſudden conſumption of pure air be the object of dread, there is as much reaſon to avoid a brewery, a furnace or a glaſs-houſe, a crowded church or theatre, as a marſh or a ſwamp. The attack of intermittent or remittent fever, ſo often taking place in conſequence of only a ſhort immerſion in noxious air, and the frequent poſtponement of the attack till one or two weeks after ſuch immerſion, while, in the mean time, the healthieſt air had been breathed, are likewiſe unanſwerable objections to Dr. C.'s hypotheſis.

(To be continued.)

Medical and Philoſophical News.

DOMESTIC.

PROGRESS OF PNEUMATIC MEDICINE.

IN our vol. i. p. 122, firſt edition, we announced the deſign of an eſtabliſhment to cultivate pneumatic philoſophy experimentally, and to diſcover its relations to medicine. In vol. iii. p. 305, of our Repoſitory, we mentioned Mr. Watt's account of the breathing of dephlogiſticated nitrous air; and, in p. 423 of the ſame volume, we inſerted an abſtract of the remarkable experiments made on that aëriform ſubſtance.

In addition to theſe, the philoſophical world has now before it an octavo volume of nearly ſix hundred pages, publiſhed by the ſuperintendant of the Medical Pneumatic Inſtitution. This is Mr. Humphry Davy, a man of much originality and philoſophical acumen, and poſſeſſed of as much, or more, intrepidity in ſcientific inquiries than Roſier himſelf. The title of the work is, "*Reſearches, Chemical and Philoſophical, chiefly concerning Nitrous Oxyd, or Dephlogiſticated Nitrous Air.*" It is replete with novel and intereſting matter, which is not of a general or deſultory kind, but is engaged in the details of experiments, and the minutiæ of calculation.

To this inveſtigation, Mr. Davy informs his readers, he was led by *Dr. Mitchill's attempt to explain the phenomena of contagion*; in which the Doctor originally conjectured, that ſome of the ſymptoms of endemic diſtempers were induced by this modification of azote exhaling from corrupting ſubſtances, and infecting the atmoſphere. To this belief Dr. Mitchill had been led by the experiments of Dr. Prieſtley and Meſſrs. Trooſtwyck and Deimann, on the fatal effects of that ſpecies of air, when breathed by animals; and, connecting them with ſome appearances during the prevalence of the yellow fever in New-York in 1795, threw the whole, in the form of a little eſſay, before the learned world, for conſideration, and for promoting inquiry. And if this early ſpeculation on the ſubject had been followed by no other conſe-

quence than ſuch a train of inveſtigation as Mr. Davy has made, Dr. Mitchill might feel highly gratified that he publiſhed it, and that Dr. Beddoes cauſed it to be reprinted in his Collection of Obſervations on the operation of factitious airs.

The eſſay which thus turned Mr. Davy's attention to theſe elegant and inſtructive experiments on dephlogiſticated nitrous air, was, however, ſo little thought of by Dr. Mitchill himſelf, that though it has been much in requeſt ſince the few copies originally printed were expended, he never conſented to give a ſecond edition of it.

This indifference to a republication aroſe from a conviction that the ground he had firſt taken was by far too narrow, and that the ſubject was of vaſtly greater extent than he had originally ſuppoſed. He became convinced that oxyd of azote was neither copious nor active enough to produce all the effects he witneſſed in ſeaſons of peſtilence; but grew more and more ſuſpicious, that a more highly oxygenated form of that ingredient in animal and vegetable matter called *azote*, might be the occaſional agent in exciting many forms of febrile diſtempers.

Finding, about that time (1796), that the *nitrous acid*, and not merely *water*, was procured by Dr. Prieſtley (an authority high in all chemical inveſtigations), by incorporating oxygenous and hydrogenous airs, he began to doubt whether there was any ſolid reliance to be placed upon the experiments pretending to explain the compoſition of water and of nitrous acid; and, in a letter to one of his correſpondents, ſoon after, expreſſed this ſentiment: "If, by adopting the French nomenclature, and by building upon facts which *have been ſaid* to be firmly eſtabliſhed, I have been led into any miſtakes, either of language or ſcience, they will, I truſt, be found to interfere with ſome part of my *particular reaſoning* only, but not to militate at all againſt my *general concluſion*." (See Med. Rep. vol. i. p. 266, firſt edition). Dr. Prieſtley adheres to the correctneſs of this experiment to this day, in his late tract on the refutation of the compoſition of water.

Conſidering it by no means certain what the preciſe chemical conſtitution of *nitrous acid* was, he left that point to be ſettled by the experimenters, who, notwithſtanding all their ſkill in aſcertaining by the balance the fractional parts of a grain in the gaſes they mingle, are ſtill at variance with each other. Some of them, men of great conſideration and experience, even believe *azote* itſelf to be a compound. To avoid all this controverſy, and in conformity to what he had obſerved of a connection

between putrid effluvia and endemic diſtempers, in American ſummers and autumns, he altered the nomenclature, and adopted a ſet of terms to expreſs his own ideas, leaving the former phraſeology to the diſputants, who appeared to him likely to continue diſputing as long as they adhered to their ill-choſen manner of expreſſion. And all that he intended to expreſs, in this caſt of language, was, that during the corruption of the *lean* and ſome other parts of animal and other organized matter, an acid was ſometimes formed and extricated, which was often injurious to the health, and deſtructive of the life of human beings. This acid he called *ſeptic*, becauſe produced by putrefaction; and not *nitrous*, as it had never been, as yet, a conſtituent part of *nitre*. The influence of this acid in its *original form*, and *before* its connection with other acidifiable baſes, or with alkalies, he was convinced, is a very powerful, frequent and extenſive agent in nature. But if from this it is interpreted that Dr. Mitchill ever meant, or now means, the *artificial* products of the laboratories, called nitrous and nitric acids, nitrous gas, &c. they quite miſunderſtand him. He knew, from the beginning of his inveſtigation, the qualities of theſe fluids, the greater part of which can only exiſt in *cloſe veſſels*, too well to ſuppoſe they were the modifications of azote which could poſſibly produce any laſting effects in *the open atmoſphere*. Things known but in the laboratories, have but a ſmall operation out of doors. And it is not a little curious to read Mr. Davy's avowal (though there was no need of making it) concerning the ſubſtance which is the principal ſubject of his book, that "there are no reaſons for ſuppoſing nitrous oxyd is formed in any of the proceſſes of nature." (P. 231).

Mr. Davy obſerves (p. 145, note), that *carbonic acid* and *ammoniac* are both products of animalization (he might have added *vegetalization* too); why could he not have ſaid that *ſeptic acid* was alſo? This ſeptic acid, as underſtood by Dr. Mitchill, produces its deleterious effects quickly after its production, and *before* combination with pot-aſh. Mr. Davy's experiments are made upon the *altered* acid and its modifications, *after* its ſeparation from that alkali, when its native virulence is very much diminiſhed. The almoſt endleſs modifications and mitigations of ſeptic acid by nearly every thing that it comes in contact with, were, in 1797, pointed out by Dr. Mitchill, in his paper on manures. Mr. Davy's excellent and admirable book bears witneſs, throughout, of the diminution of its virulence after frequent mixtures and decompoſitions.

Mr. Davy has attempted to prove, that one hundred grains of nitrous oxyd conſiſt of about 63 parts nitrogen (as he calls it), and 37 oxygen, exiſting, *perhaps*, in the moſt intimate union which thoſe ſubſtances are capable of aſſuming; that nitrous gas is compoſed of about 44 nitrogen and 56 oxygen; that nitric acid (p. 329) conſiſts of about 1 nitrogen and 2. 3 oxygen (is this in the ratio of 33 nitrogen and 67 oxygen to the 100, or what is it?) that nitrous acid is a mixture of 68. 06 nitrous gas, combined with 31. 94 oxygen, or of nearly 30 nitrogen and 70 oxygen (p. 19); and that atmoſpherical air, which is the leaſt intimate of the combinations, is made up of 73 nitrogen and 27 oxygen.—In our preſent vol. p. 83, we ſtated the reſult of Berthollet's late experiments, that the atmoſphere contained but 22 parts of oxygen in the 100: in our vol. iii. p. 382, we gave an account of Prieſtley's later experiments, that nitrous acid was made by ſyntheſis, without a particle of nitrogen: in the ſame volume, p. 213, we inſerted Profeſſor Woodhouſe's experiments, to ſhow that nitrous gas can never be obtained of one and the ſame degree of ſtrength: in Mr. Lee's experiments, ibid. p. 29, the difference between nitrous gas and a mere mixture of nitrogen gas and vital air are pointed out: and we now obſerve, that from the diſcordant relations of the beſt chemiſts there muſt be great uncertainty and much gueſs-work in theſe matters, notwithſtanding the fair and ſpecious ſhow of decimals, which ſum up ſo nicely.* The whole myſtery is very far from being unfolded yet. It ſeems, indeed, to reſt about where it was. None of the uncertainty is removed by Mr. Davy's experiments.

Mr. Davy has made many experiments upon nitrous oxyd, and diſcovered a great number of new facts. He procures it

* Mr. Davy is very free in pointing out the fallacies and miſtakes of Humboldt, Vauquelin, the Dutch chemiſts, &c. they, in their turn, might poſſibly think there were ſome miſtakes in the *Reſearches, &c.* not corrected in the column of errata. For example: In p. 19 it is aſſerted that 100 grains of *nitrous* acid, and this Mr. Davy's ſtandard acid too, conſiſt of 29. 9 parts nitrogen and 70. 1 oxygen; and in his *table of approximations*, p. 565, he ſtates that *nitric* acid is compoſed of 29. 50 nitrogen and 70. 50 oxygen in the 100—A very trifling difference indeed, amounting to but a few decimal parts of an unit;—too trifling a difference, ſome have thought, to be true; eſpecially ſince, in p. 329, he tells his readers, quite as gravely, that *nitric acid* is compoſed of about 1 nitrogen and 2. 3 oxygen; a ratio differing four or five WHOLE NUMBERS from what he ſtates in the other place; that is, *nitric* acid differs from itſelf more, by ſeveral integral parts of an hundred, than it does from *nitrous* acid. But printers will commit blunders. The alteration of a few cyphers will remove this ſeeming contradiction in the ſecond edition.

for common uſe from nitrate of ammoniac, ſlowly decompoſed by the heat of a ſpirit lamp. Among other qualities, it readily unites with water, with alkohol, ether, and eſſential oils, and even with alkalies, into a *new* claſs of mixed ſalts. The reſearches are four in number, and the moſt valuable matter is contained in the *third* and *fourth*. They treat of its effects upon breathing animals, quadrupeds, birds, amphibia, fiſhes, inſects, and upon human beings. Theſe could generally live in it longer than when immerſed in water or inflammable air; whence it appeared to act directly upon the conſtitution, imparting, for a time, ſomewhat capable of preventing the *immediate* extinction of life. But they died infinitely ſooner than in common air or oxygenous gas. He ſhows that nitrous oxyd is abſorbed and combined with venous blood in the lungs; and that, in common reſpiration, a conſiderable portion of nitrogen (contrary to the experiments of others) enters into the circulating ſyſtem too. By breathing *nitrous oxyd*, after having made two inſpirations and a complete expiration of hydrogen, Mr. Davy put his own life in jeopardy. He run a greater riſk in making three inſpirations and expirations of *hydrocarbonate*; and, in an attempt to reſpire *nitrous gas*, would undoubtedly have been killed, had not the epiglottis ſpaſmodically cloſed by its ſtimulus, and excluded it. On admitting common air into his mouth, after this bold trial, aëriform nitrous acid was inſtantly formed, which burned his tongue and palate, injured his teeth, and produced an inflammation of the mucous membrane.

We gave ſome account of the effects produced by nitrous oxyd in our vol. iii. p. 423, to which we refer our readers, as they are an anticipation, in part, of the caſes reported by Mr. Davy in the work before us. In its pure form it frequently produced increaſe of muſcular ſtrength, pleaſurable feelings, high, exquiſite, ſublime and indeſcribable emotions; mounting from delicious tremors of the nerves, through all the gradations of thrilling ſenſation, up to ungovernable geſtures, ecſtacy and tranſe. In its pure form, ſo violent are its effects, that "the moſt robuſt are unable to reſpire it with ſafety for more than five minutes" (p. 553). Some were much incommoded by two or three inſpirations (p. 498, 531). Involuntary laughter, vertigo, intoxication, fainting, tremors, delirium, ſuſpenſion of the ſenſes of ſeeing, hearing, feeling, and, ultimately, the power of volition itſelf (p. 504), are among the ſymptoms ſometimes induced by the reception of it into the lungs. Dizzineſs, fulneſs of the head, luminous flaſhes in the

eyes, incapacity to ſpeak, torpor and drowſineſs, increaſe of irritability, dimneſs of ſight, hyſterical and nervous affections, privation of voluntary motion, renewal of old pains, ſleepineſs and light-headedneſs, were alſo induced, on ſome occaſions, by this ſingular agent. *Three* or *four* reſpirations frequently produced ſome of theſe extravagant and diſtempered emotions.

Making all the deductions which a calm mind may think requiſite from the highly coloured and glowing account given of theſe proceedings, they are ſtill very curious in themſelves, and preſent ſome grand and inſtructive analogies. Candid ſeekers after truth will rejoice in tracing a few of them.—The *dark purple colour* (Reſearches, p. 347 & ſeq.) of the venous blood of animals deſtroyed by Mr. Davy's gaſeous oxyd of ſepton (nitrous oxyd), agree very well with the dark purpliſh complexion of blood drawn from the veins of perſons under the influence of the poiſon from putrefying beef, fiſh, &c. forming the yellow fever deſcribed by Dr. Rand (Med. Rep. vol. ii. p. 473, firſt edition). The *extravaſated blood*, *brown*, *red*, and *purple* and *dark* ſpots of the lungs mentioned by Mr. Davy, induced by the ſame agent, have a near reſemblance to the condition of thoſe organs in Drs. Warren and Rand's diſſections of human bodies deſtroyed by the locally-produced peſtilential exhalations in Boſton (ibid. p. 251). The *dark colour of the muſcular fibres* obſerved by Mr. Davy, with his gaſeous oxyd, *and their diminiſhed irritability*, correſpond with ſeveral diſſections, witneſſed by Dr. Paſcalis, of perſons killed by expoſure to ſeptic gaſes, and with his obſervations on their effects during the rage of yellow fever in Philadelphia (ibid. p. 347, 348). The like ſeems to obtain as to the hue of the *brain*. And the agreeable ſenſations experienced by thoſe who breathed this ſpecies of air, bring to mind the "unuſual vivacity," which ſeemed, as Dr. Ruſh obſerved in 1793, to have been produced "by a leſs quantity of the contagion acting as a cordial upon the ſyſtem;" and which that admirable writer records among the premonitory ſigns of yellow fever (Account of Yellow Fever, p. 36). This amounted, in ſome, to *preternatural excitement* and morbid ſtrength. In ſome, death ſeemed to inſinuate itſelf into the ſyſtem with all the gentleneſs of natural ſleep, and ſome expired with ſmiles on their countenances (ibid. p. 78).—Others have obſerved extraordinary exhiliration from the effects of peſtilential poiſon.

It is matter of importance to find this powerful ſpecies of air, which ſo readily diſturbs the functions and deſtroys the

lives of animals, like ſeveral other noxious gaſes of the ſame family, is capable of attraction by alkalies. Mr. Davy has enlarged our knowledge of the action of theſe bodies, by proving THAT NITROUS OXYD CAN BE COERCED BY POT-ASH AND SODA, AND, HE IS INCLINED TO THINK, BY AMMONIAC AND LIME (p. 262 & ſeq.). He propoſes to call ſuch a combination "nitroxis" of pot-aſh, ſoda, &c.

The managers of the Pneumatic Inſtitution are of opinion, that the excitement produced by nitrous oxyd is not followed by collapſe or indirect debility; that its high ſtimulation has no weakneſs conſequent upon it. This, we muſt own, is paſſing ſtrange, when, on the face of their own narrative, it appears to be ſo violent a ſtimulus, and ſingularly to diſorder the animal machine. Experiments are making on its medicinal powers. Conſiderable advantages are ſaid to have been already derived, and much more benefit is anticipated. Their American brethren heartily wiſh the enterpriſing and indefatigable gentlemen ſucceſs in their labours. Such zealous and amicable co-operation between the votaries of ſcience on both ſides of the Atlantic promiſes abundance of good.

POT-ASH FORMED DURING THE INCINERATION OF WOOD, FROM ITS ELEMENTS.

If the fixed vegetable alkali is an elementary material, it might be expected to ſhow itſelf after the decompoſition of plants by *putrefaction* as well as by *incineration*. Yet the American foreſts, where immenſe quantities of timber are rotting down, afford no evidence of this. On the contrary, the trunks of the largeſt trees, as they undergo gradual decay upon the ground, give no ſign of pot-aſh. Frequently, in the progreſs of decompoſition, the annual circles are ſo detached from each other as to be eaſily peeled off, and the coheſion of the wood ſo much leſſened, that the blade of a knife or of a ſword can be thruſt in toward the medullary part their whole length. Now, there is no ſaline efflorescence on this rotten timber in dry weather: nor is there any alkaline taſte; nor any pot-aſh to be obtained by macerating it in water: nor are vegetable blues or purples in the leaſt rendered green by dipping in ſuch water. Indeed, the manufacturers of the article, which is one of the great ſubjects of export from New-York, know, that in clearing the wilderneſs, *the trees, in order to afford pot-aſh, muſt be burned*; if they are ſuffered *to rot, no alkali can be procured*. On the contrary, the rotten wood contains

an acid.—Did pot-aſh pre-exiſt in the wood, why ſhould it not be evolved by putrefaction? Theſe conſiderations, and the analogy of ammoniac, lead to a perſuaſion, that this alkali and ſoda are compounds. Whether, as ſome have aſſerted, carbon and azote are the ingredients, or whether there are other conſtituent parts, are points not as yet ſettled.—From their compound nature, a conſideration reſults worthy the attention of experimental chemiſts, in reſpect to their union with acids, and the conſtitution of neutral ſalts. The acid of putrefaction furniſhes an example. This pernicious offspring of corruption very readily aſſociates with moſt natural bodies, except ſilicious earth, or is decompoſed by them; becoming, oftentimes, a complicated and ſtrange production. Moſt of theſe endleſs modifications of the ſeptic acid combine, more or leſs forcibly, with pot-aſh. And it is well known, the acid procured by diſtilling and decompounding ſalt-petre poſſeſſes a number of qualities which it is not known to poſſeſs *before* its connection with pot-aſh in the form of nitre. There is reaſon to believe, therefore, that this alkali itſelf undergoes ſome decompoſition, and proportionally modifies or affects the *ſeptic* acid, ſo as, on its diſengagement, to exhibit itſelf in that modified and diſguiſed appearance called the *nitrous*. Both manufacturers and conſumers of pot-aſh know how exceedingly it varies in quality. And our inſpectors themſelves, in New-York, acknowledge and lament the want of a juſt ſtandard to determine its ſtrength. They frequently find a difference in alkaleſcency, pungency and external appearance, which they know not how to explain; but the beſt explanation of which is, that the different ſamples or parcels vary in the proportions and combinations of their conſtituent elements.

ANTISEPTIC POWER OF CALCAREOUS EARTH.

Naturaliſts have often remarked, that the bones of animals were preſerved, in a tolerably entire condition, in the calcareous rock of Gibraltar. America furniſhes two facts that are no leſs remarkable. The bones of the Megalonyx, or Great Claw, deſcribed by Mr. Jefferſon, were found in one of the *lime-ſtone caverns* in Tenneſſee, covered with the ſeptite of lime, the acid of which was probably derived from its body, in part, as it putrefied, and other calcareous matter. And the other enormous bones of the Megatherium, of which Mr. S. Miller has given an account, exiſt, in great numbers, in Ulſter county, within one hundred miles of the city of New-York, bedded in marle, and found when that calcareous

material is dug out for the purpoſe of manuring land. It is remarkable of theſe bones, that, after being removed from their calcareous graves, they ſoon begin to crumble by expoſure to the atmoſphere. The teeth particularly, which are whole and ſound when taken out of the marle, ſoon turn black, crack, loſe their enamel, and crumble ſlowly to pieces. Without ſuch a preſervative, it is preſumable theſe animal remains would have been *decompoſed ages ago.*

HYDROGEN AN INGREDIENT IN NITROUS OXYD.

Nitrous oxyd can be obtained from nitrate of ammoniac. During its decompoſition by moderate heat in a retort, that ſalt is almoſt reſolved into a liquid and an aëriform fluid. The gas is the nitrous oxyd, which is reported to conſiſt of 37 parts oxygen and 63 nitrogen in the 100. It is formed by a decompoſition, as is thought, both of the nitric acid and of the ammoniac; part of the oxygen of the nitric acid joining with the hydrogen of the ammoniac to form water, and the nitrogen of the ammoniac uniting with the reſidue of the nitric acid to make nitrous oxyd (Reſearches, p. 104). But this theory is evidently not correct: for Dr. Prieſtley's experiments ſhow that phlogiſton, or the baſe of inflammable air (Med. Rep. vol. iv. p. 103), is a conſtituent part of phlogiſticated or azotic air. This concluſion ſeems to be countenanced by the appearances, related by Mr. Davy, on burning a candle in nitrous oxyd. "It burnt with a brilliant and crackling noiſe; and, before its extinction, *the whole inner flame* became ſurrounded *with an exterior blue one.*" This latter was evidently a portion of *hydrogen* extracted from the nitrous oxyd, and undergoing a new combination. And to the preſence of this ingredient does the nitrous oxyd probably owe its maddening and intoxicating quality, as wine and alcohol do. It is even likely a portion of the *hydrogen of the ammoniac* unites with the nitrous oxyd.

ABSTRACT OF DR. CURRIE'S WORK ON WATER AS A REMEDY.

A neat and judicious abridgement of the publication of Dr. James Currie, of Liverpool, on the medicinal uſe of water of different temperatures, has been made by Mr. Benjamin Vaughan, of Hallowell, in the Province of Maine. A ſenſible exhibition of the leading points of doctrine and

practical direction, in works like this, in a cheap and popular manner, is a very deſirable object in a country where the original is rare and hard to be procured.

EXPERIMENTS TENDING TO SHOW THAT AZOTE IS A COMPOUND OF HYDROGEN AND OXYGEN.

A memoir on the compoſition of azote has been publiſhed in the French Annals of Chemiſtry. The author is Dr. Girtanner. He is of opinion that azote is not an element, as has been generally underſtood ſince the publication of the table of the nomenclature, but a compound; and that it conſiſts of the ſame ingredients that water does, varying only in their proportions; thus—water conſiſting of 85. 66 oxygen and 14. 34 hydrogen, and azote 79 oxygen and 21 hydrogen.

The proofs on which this idea is founded are ſtated in the London Monthly Magazine, vol. ix. p. 585, as follows: 1. It was obſerved by Mr. Van Hauch, that if a porcelain tube was heated red-hot, and ſteam paſſed through it, the aqueous vapour undergoes no alteration; but that if it be filled with tin filings previous to paſſing the ſteam through, the tin becomes oxydated, and oxygen and azote paſs into the receiver. The ſame effects take place if lead be made uſe of inſtead of zinc. 2. Mr. Van Hauch filled a porcelain tube with black oxyd of manganeſe, and heated it ſtrongly till no more oxygen came over: he then paſſed ſteam through it, and obtained, at firſt, pretty pure oxygen, and afterwards azotic gas. 3. Dr. Pearſon, in the decompoſition of water by the electric ſpark, always obtained azote, as well as oxygen and hydrogen. 4. Dr. Pearſon, in recompoſing water by the combuſtion of oxygen and hydrogen, by means of the electric ſpark, obtained water and azote. 5. In the combuſtion of oxygen and hydrogen, when this laſt is in exceſs, nitric acid is obtained.

Girtanner's own experiments are ſuppreſſed, with a remark of the editor, that he is not much worthy of credit. We believe the ſubject to be exceedingly important in ſcience, and are ſorry Girtanner's experiments have not reached us. But we are happy to add, however light they may be deemed, the philoſophical world will find ſterling weight in the following ones of Dr. Prieſtley.

1. He is convinced, from many experiments, that azote is not a ſimple ſubſtance, but a compound of the baſes of inflammable and dephlogiſticated airs, and capable even, in ſome

caſes, of being generated from inflammable air alone (Doctrine of Phlogiſton eſtabliſhed, ſect. 11).

2. From many more experiments, he is convinced that the *whole* of *any given quantity of water* is converted, by frequent freezings, to *phlogiſticated air* (Med. Rep. vol. iv. p. 17 & ſeq.)

3. Various other experiments made by him, and publiſhed in 1793, tend to prove that water, when turned to *vapour*, is always in *part* converted into phlogiſticated air; and, by continuing the proceſs, *the whole* is ſuſceptible of a ſimilar change.

4. When dephlogiſticated and inflammable airs are confined in a ſuitable veſſel, and the electrical ſpark paſſed through them, a highly phlogiſticated nitrous acid is inſtantly produced; and the purer the airs are, the ſtronger is the acid found to be. When phlogiſticated air is preſent, it remains unchanged, unleſs there is a conſiderable deficiency of inflammable air; in which caſe it unites with the phlogiſticated air, and forms the ſame acid: whence it is inferred, inflammable and phlogiſticated airs have a great ſimilarity of conſtitution. If there is a redundancy of inflammable air no acid is formed.

5. Where water is formed by combining the two airs, a quantity of phlogiſticated air is always produced as a matter of ſurpluſage in the experiment.

6. In ſome caſes, iron filings and ſulphur, immerſed in mercury or water, or placed in a vacuum, yield phlogiſticated inſtead of inflammable air.

From ſuch experiments, and a variety of others, it is fairly to be inferred, that there is a nearer reſemblance of *water* to *azotic* air, and of *this* to *hydrogenous* gas, and of both to *nitric acid*, than the adherents of the French doctrine are willing to allow. Indeed, if *azote is a compound of hydrogen and oxygen*, as both Dr. Prieſtley and Girtanner ſeem to have rendered probable, a large part of the controverſy can be eaſily ſettled, as all theſe ſubſtances are but different modifications of the ſame elements.

Dr. Prieſtley has long ago ſhown by what proceſs in the economy of nature, the *oxygenous portion* of the atmoſphere was reproduced and reſtored; to wit, by the operation of light on growing vegetables. He has now preſented as notable a diſcovery, which is the counterpart of the former, of the means by which its *azotic part* is renewed and kept in equilibrium, viz. by the converſion of water to phlogiſticated air,

when it is reduced, by *any means*, into a ſtate of vapour. (See Med. Rep. vol. iv. p. 20).

The philoſophical men who had implicitly adopted the doctrines of the French ſchool, as originally promulgated, were ſo numerous, ſo poſitive, ſo impetuous, and ſo perſevering, as nearly to bear down, at their firſt charge, every kind of oppoſition. Dr. Prieſtley, almoſt alone, or with a handful of adherents, has remained firm in his citadel, and refuſed both to ſurrender and to capitulate. His conduct has been eminently heroic, and honourable to him. He is plentifully ſupplied with the beſt of intellectual ammunition and proviſion. And as there is no probability that his adverſaries can ſucceed, either by ſtorm or blockade, the only reſource they have left is, on conviction of the weakneſs of their force, or the injuſtice of their cauſe, to raiſe the ſiege, and march off to their proper provinces.

DOUBTS CONCERNING THE PLAGUE IN EGYPT.

The philoſophers and phyſicians who accompanied Bonaparte in his expedition to Egypt, in 1798 and 1799, have brought home very little information concerning that diſeaſe which the Europeans ſpeak of with ſo much terror, THE PLAGUE. Mr. CERESOLE, one of the phyſicians of the army, relates that the word *koubeh*, in Arabic, which is tranſlated *plague*, is a generical denomination applied to all very violent and malignant diſeaſes; and that he was unable to obtain ſo much information relative to *koubeh* as he could have wiſhed. DESGENETTES too, the Phyſician-General, teſtifies, that in that country they generally confound all peſtilential fevers, which are exceedingly various, and form a genus of themſelves, with the plague properly ſo called; which is a very circumſcribed ſpecies, but leaves the queſtion to be decided by future obſervations. The truth is, there is no proof of the exiſtence of ſuch a ſpecific diſeaſe as the *peſtis* of the noſologiſts, and the *plague* of the commercial nations of Europe. The accounts they give of it are imperfect in ſome reſpects, and fictitious in others. The fevers of Aſia and Africa, like thoſe of America, take on an hundred different forms, ariſe from the like cauſes, and die away under ſimilar circumſtances.—If buboes, and hæmorrhages, and mortifications, are criteria of the plague, then we certainly have *plague* in the United States. But we are warranted, by much experience and obſervation, in concluding that yellow fever is not *ſpecifically*

contagious, but is excited by naſtineſs and corruption both within and without the body. The ſame we believe to be equally true of the Aſiatic plague, which has been ſo fearfully ſwelled and exaggerated in deſcription. Read what the philoſopher of Maroſtica has left on the ſubject of local cauſes of diſeaſe in Egypt, more than two centuries ago, and judge.

Proſper Alpinus (De Med. Ægyptior. lib. i. cap. 13) ſays, an almoſt infinite number of the Egyptians, impelled by poverty and want, fed upon the worſt kind of aliment, and drank muddy and putrid water, at Cairo. The Mahometans uſed wine very commonly, and got drunk as well as the Chriſtians and Jews. The houſes of that city are very high, and are conſtructed ſo as to ſhelve over the ſtreets, and exclude ſunſhine and light (cap. 6). At Alexandria peſtilential fevers happened almoſt every autumn, and were more deſtructive to *ſtrangers* than to the *natives* (cap. 14). P. Alpinus takes up almoſt a page in enumerating local cauſes of this diſeaſe, ſuch as the corrupt exhalations from Lake Mareotis in its neighbourhood; from low places and ſunken holes in the city itſelf; from the water of the Nile conducted to the town, drawn off into ſtagnant reſervoirs, kept during the year for family uſe, ſometimes growing poiſonous before it was conſumed, and uſed in this condition for cookery and drinking, &c. (p. 52). He mentions the canal of Cairo as an abominable nuiſance, as becoming charged with abundance of putrefying things, changing from green to black, and becoming horribly ſtinking, and deſtroying the lives of perſons living near it. He deſcribes the diet of the poor (in a ſociety where almoſt all are poor) as conſiſting of muddy and corrupted water, and half rotten beef, camel's fleſh, fiſh and cheeſe, eaten by them becauſe they are cheapeſt, and becauſe they can buy no other. —After all this, Alpinus admits, though with a ſeeming reluctance, that the peſtilential contagion may be engendered from putrefaction, in certain places, after the waters of the ſwollen Nile have withdrawn. But he thinks, upon the whole, the contagion is commonly imported, either from Greece, Syria or Barbary, and that the contagion from Barbary is by far the worſt!! It rages from September to June, and then ſtops ſpontaneouſly, when the Eteſian or cool northerly winds begin to blow.—Leave out the contagion, and this account is philoſophical and correct.

Obſervations in Natural Hiſtory, made in the Summer and Autumn of the Year 1800, *by* S. L. M.

1. *Account of the Motions of the Hedyſarum Gyrans, or moving Plant, deſcribed in Botanic Garden, Part* II. *Canto* iv. *l.* 335 *& ſeq.*

In the note to this paſſage, Dr. Darwin informs his readers "that its leaves are continually in ſpontaneous motion; ſome riſing and others falling; and others whirling circularly by twiſting their ſtems. This ſpontaneous movement of the leaves, when the air is quite ſtill and very warm, ſeems to be neceſſary to the plant, as perpetual reſpiration is to animal life." From this deſcription we were led to believe that *all* the leaves of this remarkable plant were to be ſeen in a riſing, falling, or whirling motion; and probably other readers of the paragraph are led to think ſo too. Our curioſity was excited to ſee this vegetable in a growing ſtate. Some ſeeds were procured and ſown. From theſe ſprang ſeveral thrifty plants. As they grew we looked for the movements of the leaves for ſome time in vain; but, at length, we diſcovered them. They fell ſo far ſhort of what we had been led, from the deſcription, to expect, that we were not a little diſappointed. This hedyſarum produces two kinds of leaves, the one *large* and the other *ſmall*. The former are, when full-grown, about $2\frac{3}{4}$ inches long, and $\frac{3}{4}$ wide, and are ſupported by a foot-ſtalk of one inch in length. Theſe conſtitute the principal foliage of the plant, and we expected to ſee them fanning themſelves, and panting for breath, during the heats of July, Auguſt and September (1800). But in *theſe* no ſuch changes of poſition could be ſeen, nor any other alteration of place different from other plants. They, indeed, cloſed themſelves to the ſtems at night, and in cool and ſtormy weather, as happens to a multitude of other vegetable ſpecies. Mere elevation and depreſſion, at ſuch times, were all the motions we could diſcern in the large leaves. The *latter* kind of leaves grow out of the foot-ſtalk about $\frac{1}{4}$ of an inch below the inſertion of the large one. Theſe are not more than $\frac{1}{2}$ an inch long, and $\frac{1}{8}$ of an inch broad. Generally, each foot-ſtalk ſupports *two* of theſe ſmall leaves. Not unfrequently, however, there is but *one*, and ſometimes there are *none*. Both kinds are well delineated in Dr. Darwin's plate, which is a very good repreſentation of the plant. *The ſpontaneous motions are performed by theſe leaflets, which move backwards, forwards, and ſomewhat circularly, like the ears of horſes or ſheep.* And with this

limitation ought the celebrated gyrations of this ſpecies of the numerous family of hedyſarum to be underſtood. So much is the account exaggerated, that ſome patience and nicety of obſervation are neceſſary to diſcern them.

2. *Account of the Sun-dew, or Droſera, deſcribed in the ſame Work, Part* II. *Canto* i. *l.* 239 *& ſeq.*

"The leaves of this marſh-plant," writes this philoſophical poet, "are purple, and have a fringe very unlike other vegetable productions; and, which is curious, at the point of every thread of this erect fringe ſtands a pellucid drop of mucilage, reſembling a ducal coronet. This mucus is a ſecretion from certain glands; and, like the viſcous material round the flower-ſtalks of the ſilene (catch-fly), prevents ſmall inſects from infeſting the leaves." The droſera rotundifolia growing in my ſwamp is rooted in peat-moſs (ſphagnum paluſtre), and is very ſmall. The leaves are green, and not only the margin, but the *whole upper ſide* is thickly beſet with hairs or briſtles of a *red* colour. Upon the ſummit of each of theſe, in the vigorous ſtate of the plant, there is formed a globule as clear as cryſtal. Forty or fifty ſuch *pellucid* balls, ſuppported by *red* pili, growing out of a *green* leaf, make an uncommon and beautiful appearance. Theſe globules conſiſt of a tenacious liquid, which entangles the legs of ants, flies, or other ſmall inſects which attempt to travel acroſs the leaves. Whenever this happens, a leaf which is naturally concave on the upper ſide, ſeems to form a more conſiderable hollow than before; the conſequence of which is, that the briſtles are made to converge in a degree proportioned to this concavity; and the unfortunate little creature is completely ſurrounded by an apparatus ſomewhat reſembling the palm of the hand, with the thumb and fingers half cloſed, and there held and inviſcated until it dies. My own obſervations correſpond with thoſe which Dr. Darwin quotes from Mr. Wheatley and Mr. Brouſſonet.

3. *Hybrid Variety of the Almond-Nut.*

Among the trees in my peach-orchard grows a thrifty young almond (amygdalus communis), which has borne fruit for two or three ſeaſons. On taſting them this year, we were all ſenſible of a reſemblance between the flavour of the kernel of the almond and that of the peach (amygdalus perſicus). And in ſome, ſoon after gathering, their peculiar bitterneſs reſembled ſo nearly that of the peach-kernel, that the former might, by an unadviſed perſon, almoſt have been miſtaken

for the latter. It was remarkable, too, that the nuts (drupa) were very hard and ſolid, like the peach-ſtone, and required ſmart ſtrokes of the hammer to crack them.—Hybrid plants have long been known to botaniſts and cultivators, and their numbers ſeem to be increaſing.—I was led to believe, according to the received doctrine of ſexes in plants, that the *fruit* and *kernel of the almond* had, in this inſtance, undergone a change by growing in the midſt of many trees of a different ſpecies. And if the pollen or fecundating powder of the peach has really wrought ſuch an effect upon the almond, is not this a new mongrel, and an additional fact in favour of the ſexual ſyſtem?

4. *Domeſtication of the Wild-Gooſe (Anas Canadenſis).*

Attempts have frequently been made on Long-Iſland to render the wild-gooſe which winters in the bays adjoining the Atlantic Ocean, a tame and domeſticated bird. Individuals of this ſpecies have accordingly been catched alive by the gunners after having been wing-broken by a ſhot, and carried home free from any other injury. When thus diſabled from flying, they become gentle, and will mate with common geeſe. They even breed together; but the offſpring is a mule, incapable of further propagation. Mr. Daniel Coles, of Oyſter-bay, has gone a ſtep beyond others in this buſineſs. He has a wild-gooſe and gander in a domeſticated ſtate, whom he keeps from flying away by taking off the extreme bones of the wings at the joint. The gooſe has laid eggs and hatched a brood of goſlings. For fear of loſing the young ones, their wings have been treated in the ſame manner; and the whole family now compoſes (September, 1800) a beautiful flock of wild-geeſe, in a domeſticated ſtate. They are as gentle as common geeſe, and live upon the food obtained about a houſe and on a farm quite as well. Mr. Coles even found that the goſlings, on the day of being hatched, ate Indian meal as readily as chickens. They are more active and handſome than the tame-gooſe; and their long necks are arched more like thoſe of ſwans. If this experiment ſhould be continued for ſeveral generations, it is highly probable the temper and habits of the breed may be changed, ſo that the deſcendants of theſe wild-geeſe may loſe their inclination to fly from country to country, and attach themſelves, like turkeys, ducks, and other birds whoſe progenitors were once wild, to the ſociety and protection of man. Should Mr. C. meet with no diſaſters, it is not improbable that the wild-gooſe will be eventually added to our ſtock of poultry.

5. *Squid, Ink-Fiſh, or Cuttle-Fiſh (Sepia).*

A beautiful ſpecies of the cuttle-fiſh is ſometimes found on the ſea-coaſt of New-York. It is about eight inches long. The tentacula, or feelers of this animal, are furniſhed with many mouths without throats, which are armed with a circular row of teeth to ſeize their prey. Theſe convey the food to the real or principal mouth, which is armed with a beak, reſembling the roſtrum of a parrot. The creature is furniſhed with a bag of black liquor for its defence againſt its enemies. When purſued by them, it ejects this fluid into the water through a particular orifice in the anterior part of its body. The water is darkened and rendered of an inky colour thereby, ſo that its adverſary is enveloped in a cloud, while the ſepia ſuddenly darting backward with a ſpring to the diſtance of ſeveral feet, makes its eſcape. It is very amuſing to view them thus employing the means of ſelf-defence. Some of the larger ſpecies of the ſepia are ſaid to be the chief food of the ſpermaceti-whale (phyſeter macrocephalus), and the likeneſſes or impreſſions of their beaks are frequently ſeen in ambergreaſe, which is ſaid, by the more intelligent of our Nantucket whalemen, to be but the indurated excrement of that animal in a conſtipated ſtate of the inteſtinum rectum.

6. *The Jerboa or Dipus.*

I have ſeen this little animal, which has been deſcribed by Col. Davis, of Quebec, in Tilloch's Philoſophical Magazine, No. 3, p. 285, and by Profeſſor Barton, in the fourth volume of the American Philoſophical Tranſactions, p. 114, in the State of New-York. It is ſometimes killed on Long-Iſland by the farmers when engaged in carting hay and corn-ſtalks.

7. *Another Inſtance of a Negro turning white.*

The change of colour which Harry Moſs has, within a few years, undergone, from black to white, has been publiſhed ſo often that few curious perſons are ignorant of it. In the town of North-Hempſtead, ſomething of the ſame kind is now to be ſeen. A young negro, named Maurice, aged 25 years, began, about ſeven years ago, to loſe his native colour. A white ſpot appeared on the right ſide of his belly, which is now about as large as the palms of two hands. Another white ſpot has appeared on his breaſt, and ſeveral more on his arms and other parts; and the ſable cloud is plainly diſappearing on his ſhoulder. The ſkin of theſe fair ſpots is not ſurpaſſed by the European complexion. His general

health is and has been good; and he has ſuffered no ſcalding ulceration, ſcabbineſs, or other local diſeaſe. The change is not the dead white of the *Albinos*, but is a good wholeſome carnation hue. Such an alteration of colour as this, militates powerfully againſt the opinion adopted by ſome modern philoſophers, that the negroes are a different *ſpecies* of the human race from the whites, and tends ſtrongly to corroborate the probability of the derivation of all the *varieties* of mankind from a ſingle pair. Facts of this kind are of great value to the zoologiſt. How additionally ſingular would it be, if inſtances of the ſpontaneous diſappearance of this fable mark of diſtinction between ſlaves and their maſters were to become frequent! They would then be no leſs important to the moraliſt and political economiſt.

8. *Clay for manufacturing Tobacco-Pipes.*

A ſmall attempt has been made at Muſqueto-Cove, in Queen's county, to manufacture tobacco-pipes. An Engliſhman, who poſſeſſes the machinery and ſkill for moulding them, has been for ſome time engaged in making trials on the different clays he can procure thereabout. On the economy or probable ſucceſs of ſuch an eſtabliſhment at this time in America, we offer no opinion. We underſtand that a former attempt failed. Be that as it may, it is agreeable information that pipes, of a tolerable quality, have been *formed of American clay*. The ſamples of the manufacture which we have ſeen, do not indicate want of talent in the artiſt; and, though rather deficient in whiteneſs and coheſion, will anſwer for common uſe. It is to be hoped, *clays of greater purity and toughneſs* will ſoon be found, and thereby afford another proof of the *reſources of our country*.

CHRONOLOGY.

The Rev. John C. Kunze, S. T. D. is preparing for the preſs, a work on chronology. It is intended to treat of the chronological character of the year which cloſes the eighteenth century, in the following points of view:—What year actually cloſes it, according to the received chronology? What year ought to cloſe it by following juſt computation? Whether the eighteenth centurial year ought to be a leap-year? What year the 1800th J. C. probably is ſince the creation? What year it may be in the anti-chriſtian period? Of the aſtronomical incidents of the year which cloſes the eighteenth century.—A ſerious and formal diſcuſſion of a ſubject which en-

gages ſuch general attention, and on which there exiſts great difference of opinion, will doubtleſs be highly welcome. We hope the learned author will gratify public curioſity as ſoon as he conveniently can.

MATHEMATICAL PRINCIPLE FOR CONSTRUCTING THE MOULD-BOARDS OF PLOUGHS.

In the 4th vol. of the Tranſactions of the American Philoſophical Society is a paper of Thomas Jefferſon, the Preſident, whoſe object is to inveſtigate, geometrically, that figure of the mould-board which will turn over the ſward and paſs through the ground with the leaſt poſſible reſiſtance, or with the ſmalleſt loſs of exertion in the team. Unfortunately, in binding up the ſheets of the book, the plate of diagrams, explanatory of the deſcription, has been left out, ſo that the piece is not exhibited as the author intended. But he has forwarded to the Preſident of the Agricultural Society *a ſet of the figures referred to in the deſcription*, together with *an elegant wooden model*, illuſtrative of the manner of applying the principle to practice. That patriotic aſſociation in New-York will thus have an opportunity of becoming fully acquainted with this noble inveſtigation of the philoſopher of Monticello.

CASTOR-OIL MADE IN NEW-YORK.

The plant whoſe ſeeds afford caſtor-oil has long been cultivated in our gardens, under the name of *head-ach-bean*, or *bug-bean* (ricinus palma chriſti). Little or no uſe had generally been made of it, other than to bind the leaves on the head for relieving pains of that part. Some perſons raiſed it under a perſuaſion it would keep moles out of gardens, and others merely for curioſity. Latterly, however, John G. Gebhard, of Claverack, has prepared the oil from the ſeeds by expreſſion; and the product appears quite as good as the beſt imported from the Weſt-Indies, with this circumſtance in its favour, that it is *cold-drawn* and *always freſher*. This is another evidence of the reſources of the United States, and the citizen who has undertaken the manufacture merits the encouragement of his countrymen, whether druggiſts, houſekeepers, or phyſicians.

GRAND SPECIMEN OF MEXICAN GOLD.

A maſs of native gold from one of the Mexican mines, which, on account of its rare ſize and beauty, was intended, by the Viceroy of Mexico, as a preſent to the King of Spain,

is now poſſeſſed by Capt. O'Brien, in the city of New-York. The metal, which is malleable, and not mineralized, is connected with quartz. The whole lump weighs 46 ounces troy. No part of it is cryſtallized. The colour of the gold is a fine yellow, with a tincture, in one part, of whitiſh, and in another of greeniſh. The quartz is white, with a duſky complexion. The maſs ſeems to be a looſe nodule, never connected with a large rock. The value of the gold it contains is eſtimated at five hundred dollars. In the collection of a prince, who can afford ſuch ſpecimens, this piece is invaluable.

MINERALOGICAL SPECIMENS FROM GIBRALTAR AND ICELAND.

The Mineralogical Society of New-York has received from Dr. Thomas Wilſon two fine ſpecimens; one of the *calcareous* rock of Gibraltar, incloſing animal bones; and the other a *ſilicious* ſtalactite from the boiling-hot fountain of Guyſar, at the foot of Mount Hecla. Our readers are referred, for particular accounts of theſe remarkable natural productions, to Major Imrie's deſcription of the former, and Profeſſor Black's analyſis of the waters of the latter, publiſhed in the Tranſactions of the Royal Society of Edinburgh.

MEDICAL SOCIETY OF NORTH-CAROLINA.

On Monday, the 16th of April, 1800, a convention of the Faculty was held in the city of Raleigh, aſſociated under a conſtitution and form of government by the name of the "*North-Carolina Medical Society*;" and the following gentlemen were elected officers for the enſuing year:

Richard Fenner, *Preſident.*
Nathaniel Loomis, John Claiborne, } *Vice-Preſidents.*
Sterling Wheaton, James Webb, James John Paſteur, and Jaſon Hand, } *Cenſors.*
Calvin Jones, *Correſponding Secretary.*
William B. Hill, *Recording Secretary.*
Cargill Maſſenburg, *Treaſurer.*

The following gentlemen were appointed to deliver diſſertations on ſome medical ſubject at the next meeting of the Society, viz. J. J. Paſteur, J. Webb, S. Wheaton, and N. Loomis.

An Eſſay on the Symptoms, Nature and Cure of the Dyſentery, was made a prize ſubject for any medical gentleman practiſing in this State, at the next annual meeting, which will be held on the firſt day of December next. The prize diſſertation muſt have annexed to it ſome cypher or emblem to identify the author, encloſed and ſealed, which will be burnt if it ſhould not be accounted worthy of the prize. The ſealed encloſure to be broken open in the preſence of the Society.

From the early patronage of the Legiſlature towards this firſt ſcientific Society of the State (having, at their preſent ſeſſion paſſed an act for incorporating it), and from the zeal and enterpriſe of the gentlemen who compoſe it, we truſt it will prove a Society of the firſt reſpectability and uſefulneſs.

SODA FORMED FROM ITS ELEMENTS BY INCINERATION.

Kelp or impure ſoda has, ſince the year 1746, been manufactured in the weſtern iſlands of Scotland. The plants which afford it are the different ſorts of ſea-weeds which attach themſelves to rocks. The principal ſpecies of theſe are, the *fucus veſiculoſus*, *nodoſus*, *ſerratus* and *digitalis*, Lin. But in order to afford kelp, theſe marine vegetables muſt, in their found ſtate, be completely burned. Profeſſor Walker, in his eſſay on kelp, publiſhed in the firſt volume of the Tranſactions of the Highland Society (Edinb. 1799), mentions, that when the *ſea-weeds become putrid* their produce of *lixivial ſalt is ſmall*; and Mr. Beaton affirms, when they are not *completely* burned, what is left in that ſtate *yields no ley* (ibid. p. 36). Mr. Jameſon, too, conſiders theſe plants, when damaged by fermentation, as yielding kelp of a very inferior quality (p. 45). It has been believed, that the ſoda obtained from theſe plants by burning, proceeds from the decompoſition of the ſea-ſalt, or muriate of ſoda, they contained before combuſtion. This, however, muſt be a miſtake; for though the fuci contain ſea-ſalt, it is in too trifling a quantity to account for the ſoda actually produced. This, therefore, muſt be formed from elementary combination taking place in the fire. But what is this combination? Perhaps the following facts will lead toward an anſwer of the queſtion. It is reported that carbon, if burned in *oxygenous* air, will be changed thereby to carbonic acid gas. It is ſaid, too, that if carbon is burned in *atmoſpheric* air, that not only *fixed air* but *potaſh* is formed. Now, atmoſpheric air differs from oxygenous air by poſſeſſing or containing a portion of *azotic* or *phlogiſticated* air. From this, then, combining with *ſomething elſe*,

is the pot-aſh formed. And the detection of *this ſomething* is the preſent object of reſearch. In the formation of *ammoniac* by diſtillation, Profeſſor Woodhouſe has ſhown (letter to Mitchill, Med. Rep. vol. iii. p. 210) how largely the azotic part of the atmoſphere contributes to the formation of *that alkali*. The evidence ſeems quite as ſtrong in favour of the fixation of *azote* in *ſoda* and *pot-aſh*. The day is probably not far diſtant, when we ſhall be informed with which of the known elements, as carbon, lime, &c. it combines; or whether a new, and, as yet, an unknown element, is concerned in the proceſs.

VACCINE DISEASE.

Matter of the vaccine diſeaſe, received in this city from Boſton, is now under trial. Many perſons are already inoculated: the progreſs of infection is favourable, and there is every reaſon to expect, that in a ſhort time this mild and manageable diſeaſe will be univerſally ſubſtituted for the ſmall-pox.

PESTILENCE.

We have thus far deferred our account of the malignant epidemic which lately prevailed in ſeveral parts of the United States, in the hope of obtaining many important documents, already prepared, but not yet come to hand. Some of theſe diſcloſe facts of the utmoſt conſequence, and ſuch as greatly confirm our opinions concerning the origin and nature of the American peſtilence. But the periodical limits of this work compel the poſtponement of them to another occaſion.

The courſe of the weather, during the ſummer and autumn, in this part of the United States, was generally favourable. A great part of the month of July was, indeed, extremely hot; but this heat was ſeldom accompanied by humidity, the chief means of rendering it pernicious. In Auguſt the change was equally unexpected and ſalutary. The heat of that month was moderate, both in degree and duration; cool days very frequently intervened; and the rains, unlike thoſe of preceding years, were almoſt invariably followed by coolneſs of the air for many days. September partook of the ſame character, neither warm nor moiſt to an exceſſive degree, but generally ſeaſonable and pleaſant.

Baltimore and Norfolk have ſuffered greatly beyond any other places in the United States, by the ravages of this malignant diſeaſe, in the late epidemic ſeaſon. Providence (Rhode-Iſland) and New-York have likewiſe experienced a ſlight de-

gree of it; but, in moſt of our other cities, from the ſingular felicities of the ſeaſon, only a few rare and tranſient inſtances of the diſeaſe were obſerved.

From Baltimore we expect to receive ſome intereſting communications concerning the origin and nature of the late epidemic. The Board of Health of that city, on the 22d of Auguſt laſt, publiſhed the following account of the diſeaſe:

"We have no heſitation in declaring it to be entirely owing to our own local ſources of filth, vegetable and animal putrefaction, and marſh effluvia, within and around that part of the city (Fell's Point), particularly the foul ſhore of the cove, and not to any imported or human contagion. This being alſo the unanimous opinion of the Faculty in that part of the city, ſtrongly points out, that the means of prevention in future, under *favour of Divine Providence*, are now in our hands, and that a rigorous and energetic execution of the nuiſance and health ordinances muſt, and, we have no doubt, will take place."

We learn that the whole number of deaths which have taken place from the epidemic in that city, from Auguſt 1ſt to October 31ſt, of adults, amounts to 978—of children, to 219: total 1197.

Of the epidemic, as it appeared at Norfolk, the following account has been obligingly tranſmitted to us by Drs. Taylor and Hansford, in a letter of 20th October laſt.

"The diſeaſe which we have experienced this year is that which it ſeems agreed to call the yellow fever; but is, nevertheleſs, the ſame we have known for a ſeries of years, altered, at different periods, in its aſpect, by a combination of unknown, or, at leaſt, undefined cauſes, but ſtill preſerving the leading features of the autumnal bilious fever. Nor with men of obſervation and experience, who diſregard bold and innovating theories, can the varieties of appearances excite ſurpriſe, or lead to confuſion.—Do the meaſles, the ſmall-pox, the pertuſſis, the cholera infantum, or any epidemics, ſhow themſelves in different years in the ſame form? Are they not ſometimes mild, ſometimes malignant, often combined with other diſeaſes, and depending, in a great meaſure, as to their tendency, upon cauſes not to be explained? The due conſideration of thoſe analogous truths would have, perhaps, prevented the ſchiſm in the treatment of the yellow fever, which has diſtracted the public mind, and diſgraced the ſcience of medicine. No intelligent perſons amongſt us believe the diſeaſe to be imported; nor was there any quarantine ordered this year, although it

was known the fever exiſted in Baltimore before it appeared here—called by them an inflammatory bilious fever. This diſeaſe, in its malignant form, always originates on the river, or on low new made grounds, and in houſes built on the docks; and, in all caſes, begins with ſtrangers and new ſettlers, affecting every one in proportion to his time of reſidence, and leaving the old inhabitants (viz. thoſe who have been twelve months conſtantly in the town, or abſent a little while only) not wholly exempt, yet proof againſt its deſtroying power. The natives of Virginia, and to the ſouth (though newcomers), for the moſt part, eſcape with life. Thoſe from higher latitudes oftener fall victims; and, with European ſtrangers, the fever is generally uncontroulable, depending more upon conſtitution, for recovery, than the aid of medicine.—We muſt, with caution, hazard an opinion reſpecting its contagious tendency. So fearleſs are the inhabitants, that the diſeaſe is, very ſoon after its appearance, by the removal of the ſick, conveyed to every part of the town, yet ſtill is confined to the ſame deſcription of perſons, unleſs a partial ſort of diſeaſe, which ſometimes affects the attendants, &c. can be called contagion. We have been told, that in the towns of the northern and eaſtern States, no perſons of any deſcription eſcape when equally expoſed.* If this be ſo, it is the only difference we perceive in the diſeaſe, as it has been deſcribed in various parts of the world."

We are authorized to expect an account of the diſeaſe at Providence, in a ſhort time, from Dr. Levi Wheaton.

A few caſes of malignant fever occurred at Philadelphia in the courſe of the ſummer and autumn. They were admitted by all to be genuine inſtances of yellow fever. The principal part of them took place in the neighbourhood of a moſt offenſive ſpot, where a great common ſewer, called the *dock*, empties itſelf into the Delaware. After fruitleſs attempts to trace the infection from Baltimore, they were aſſerted, by the advocates of imported contagion, to be the offſpring of ſome undiſcovered importation, or of the awakened fomites of the laſt year's epidemic!!

At Wilmington (State of Delaware), among the more common forms of intermittent and remittent fevers, there occurred ſeveral very malignant caſes, which wanted nothing of the character of the yellow fever (in the opinion of ſuch as doubted) but general prevalence as an epidemic.

* It is obvious that theſe reſpectable phyſicians have been miſinformed on this point; for the reverſe has been remarkably verified.

Some few inſtances of malignant diſeaſe, under the form of yellow fever, were alſo obſerved in the town of Boſton. They were univerſally aſcribed, by the phyſicians of that place, to the influence of domeſtic miaſmata.

In New-York, the late epidemic ſeaſon, compared with the malignancy and mortality of preceding years, muſt be conſidered as mild and favourable. In thoſe parts of the town which are chiefly low, damp and filthy, and which have heretofore been diſtinguiſhed for an unhealthy character, the uſual diſeaſes of the ſummer and autumn prevailed to a very great degree. Diſperſed among theſe, and confined within the ſame local boundaries, there appeared, here and there, caſes of a more malignant grade, terminating fatally after an illneſs of only a few days, and exhibiting the ſymptoms of yellow ſkin, black vomiting and ſtools, hæmorrhages, convulſions, &c. In many of the caſes of mild remittent fever, which terminated in three or five days, and in the moſt favourable manner, the tendency to malignity was indicated by the ſuddenneſs and violence of the attack, by hæmorrhages more frequent than uſual, by the copious diſcharge of black ſtools, by a ſudden ſubſidence of the force and activity of the arterial ſyſtem after violent excitement of a few hours, and a reduction of arterial pulſations, in many inſtances, to forty beats in a minute, and by uncommon debility, greatly diſproportioned, both in degree and duration, to the preceding appearances of the diſeaſe.

From a journal regularly kept, it appears that upwards of fifty perſons died of the yellow fever, in this city, in the courſe of the late ſeaſon. Beſides theſe, we find many more fatal caſes, reported under the title of bilious and malignant fevers, which no diſcerning perſon can heſitate to add to the former catalogue. Under this mode of computation, then, it will appear, that from ſeventy-five to eighty perſons, whoſe caſes were carefully obſerved and recorded, excluſive of all ſuch as eſcaped notice, muſt have periſhed of the malignant diſeaſe called yellow fever.

Theſe malignant caſes, though nearly all occurring in the low and filthy parts of the town, were remarkably detached from one another. Of the whole number above-mentioned, only two examples (ſo far as we know, and the reports have been carefully examined for this purpoſe) are to be found of more than a ſingle caſe exiſting in the ſame family. And, admitting that the caſes which every body agrees to denominate yellow fever did not much exceed fifty, it will appear,

on inſpection of the reports, that this number of caſes was diſtributed through at leaſt twenty-five ſtreets.

It has been contended, by a few perſons, that the origin of theſe malignant caſes may be found in the awakening of the reſidual fomites of laſt year's epidemic. If this had been the caſe, we ſhould expect to find the diſeaſe re-appearing in the ſame houſes and families where it had raged laſt year. But, on a compariſon of the reports of the preſent year with thoſe of the laſt, as far as they go, only a ſingle inſtance of ſuch re-appearance in the ſame houſe is found, and this is attended with circumſtances which prohibit every ſuſpicion of dormant contagion.

No attempt, with which we are acquainted, has been made to point out any channel by which this diſeaſe might have been imported from abroad. On the contrary, we are perſuaded that the quarantine law has been interpreted and executed with an unprecedented degree of vigilance, punctuality and rigour.

No propagation of the diſeaſe by contagion is pretended to have occurred beyond the limits of that portion of the atmoſphere of the town allowed by every body to have been contaminated by the exhalations of putrefaction; and within ſuch limits, it is well known, that an adequate cauſe is conſtantly in operation, independently of contagion. In the worſt caſes, the ſick were ſurrounded, as uſual, by relatives, friends, phyſicians, nurſes, &c. After death, the bodies of the deceaſed were unreſervedly handled, laid out, placed in coffins, and conveyed to graves; the bed and body clothes were delivered to waſherwomen, &c. And yet all theſe various deſcriptions of perſons eſcaped without injury, except where they happened, in the diſcharge of ſuch offices, to be immerſed in the air of thoſe parts of the town originally contaminated by the exhalations of putrefaction.

The inferences from theſe facts, in reſpect to the origin, nature and relations of our few late caſes of yellow fever, are ſo manifeſt, that it may, perhaps, ſcarcely be deemed neceſſary formally to deduce them. As it is impoſſible to trace a line of demarcation, the whole aſſemblage of epidemic diſeaſes of the ſummer and autumn of the preſent year, whether mild or malignant, muſt be referred to the ſame cauſe. If it be contended that this cauſe is of foreign growth, the conſequence will go further than the importers of contagion are willing themſelves to believe; for it muſt then follow that

we import from abroad our mild as well as peſtilential, our common as well as extraordinary diſeaſes. The irregular aſcent of our former epidemics in this city up to the malignancy of that of 1798, and the gradual declenſion ſince, altogether militate againſt the doctrine of imported contagion, and can only be accounted for from ſuch varieties of ſeaſons as are partly cognizable by the ſenſes, and partly occult and unknown.

Upon the whole, it appears that every ſucceeding year furniſhes an additional maſs of evidence of the origin of this malignant diſeaſe from domeſtic ſources. It was our deſign here to exhibit a more ample view of this ſubject; but the non-arrival of ſome communications of the greateſt importance, for the preſent, neceſſarily ſuſpends the plan.—In the next number will be publiſhed an account of a moſt formidable diſeaſe at Narbonne, in France, originating from a local cauſe; and alſo ſome notices of a peſtilential diſtemper in the interior of this State, produced by the effluvia of a fœtid pond.

LONDON MEDICAL REVIEW AND MAGAZINE.

We announce, with great pleaſure, a new periodical work, entitled, "*The London Medical Review and Magazine,*" conducted by a ſociety of phyſicians and ſurgeons in that metropolis. One of the friends and patrons of this work has lately preſented us with the three firſt volumes, containing a variety of intereſting matter, with the obliging offer of an exchange for the Medical Repoſitory; an offer which we cordially embrace, not only from the reſpect we bear to that publication, but from our wiſh to ſtrengthen the ties which connect the ſcientific inſtitutions of the old and new world.

Our limits only allow us, at preſent, to ſelect (from p. 424 of the third volume of that work) the following paper:

THE EFFICACY OF COLD IN MADNESS.

Dr. G. G. Brown, of Bath, has communicated the following account of the good effects of cold in apoplexia mentalis, or delirium ſine febre.

Within the period of theſe laſt ten years, I have met with five caſes in the courſe of practice. The two firſt caſes were abandoned by two very eminent phyſicians, who have already done ample honour to their profeſſion, and for whom I entertain the higheſt regard. Two more caſes were attended by me alone; and two moſt reſpectable phyſicians were wit-

neſs to the ſubſequent mode of treatment in the fifth caſe. After a failure of the moſt approved medicines and practice, the application of cold water to the head, aſſiduouſly perſevered in for many days, performed the cure. I well know that this is an old ſpecies of practice, and alſo that it has often proved unſucceſsful: this, however, I attribute, in a great meaſure, to the manner of uſing it, and likewiſe to the want of perſeverance in it. The method I purſued, in the firſt four caſes, was, by winding an handkerchief round the head, and keeping it continually wet by a ſponge dipped in cold water, until it produced a ſhivering fit: it was then deſiſted from, for about an hour, more or leſs, and re-applied as before. After the firſt twenty-four hours, there was no inconvenience felt in having it always kept round the head. Between thirty and fifty hours from the commencement of the application, ſobbing and ſighing came on, which have hitherto proved the criterion of the incipient return of rational ideas. This being effected, the vitriolic acid, alone or combined with the cinchona, in conjunction with the cold application, have uniformly perfected the cure. In the firſt and fifth caſe, the application was not confined to the head, but extended along the courſe of the carotid and ſubclavian arteries. From ſeven to fifteen days, where the delirium had been of conſiderable ſtanding, have been the extent of this mode of practice; although I ſhould not have heſitated continuing it a much longer time, had it been found neceſſary. Perhaps I ſhall be induced, at ſome future period, to deliver my ſentiments more fully on this ſubject, but could no longer withhold from the public a remedy I have found ſo efficacious in ſo dreadful a malady. I have alſo reaſon to believe that it will be found equally ſucceſsful in ſome other diſeaſes, not only in the head, but the trunk of the body.

APPENDIX.

ARTICLE I.

ACCOUNT *of* LARGE BONES *dug up in* ORANGE *and* ULSTER COUNTIES *(State of New-York): In a Letter from* SYLVANUS MILLER, *Eſq. to Dr.* MITCHILL; *dated New-York, Sept.* 20, 1800.

SIR,

I RETURNED, on the 10th inſt. from a tour to the counties of Orange and Ulſter. On my arrival at Newburgh, I was informed, that about twelve miles to the weſtward of that place had lately been diſcovered the ſkeleton of an animal of uncommon magnitude, and decidedly larger than that of any of which we have at this time any knowledge. The information, to me, was not ſo extraordinary as to ſome others, as I had ſeen ſpecimens of parts of like ſkeletons, about nine years ſince, when at grammar-ſchool, in the town of Montgomery, near which place there was a part of the bones dug up: but as there were now proſpects of procuring the whole of the ſkeleton, and as more bones had already been procured than I had ever as yet ſeen, my curioſity could only be ſatisfied by viewing them myſelf, and ſeeing if there was any difference, in magnitude or form, from thoſe formerly obtained, and to obſerve what might have been the nature, ſize and qualities of the huge beaſt when alive. Theſe objects could now be better aſcertained, as the whole of the ſkeleton would probably be obtained. Unfortunately, however, this could not be effected time enough for me to ſee the ſkeleton entire. There remains, however, no doubt, but that the reſidue of the bones are there, and nothing but want of exertions, or means to defray expenſe, will hinder the whole of them from being procured.

The places where theſe ſkeletons have been diſcovered are generally called marl-pits, and are low, ſunken places, very wet and miry. The hole which had been made to procure this ſkeleton lately diſcovered, was ſo nearly filled with water, that the nature of the earth, and the colour, could not be aſcertained, otherwiſe than by the earth on the banks or borders of the hole. The draining of theſe places has been only

attempted, for the benefit of the farmer, about nine or ten years, before which time they were either unknown as affording good manure, or not in uſe in the neighbouring country. The bones here diſcovered lay buried about ten feet under this marle and earth, which generally conſiſts of five different *ſtrata* —the firſt is the common earth found in low meadows; the ſecond a very black and rich earth, and is deemed good for manure; the third a ſmall ſtratum of blue clay; the fourth a ſtratum of white marl; and the fifth a ſtratum of grey or black marl; at or near the bottom of which theſe bones are diſcovered, and ſome of them ſunk into the earth ſome inches below the marl. It may be proper to remark here, that theſe marl-pits (as they are generally called by the farmers) are very numerous, and ſome of them large and extenſive where theſe ſkeletons have been found, and that a very ſmall proportion of them have been ditched or drained off, ſo that the farmers might be able to procure the manure, to the better cultivation of wheat, corn, &c.

The bones which were lately diſcovered appear of the ſame ſpecies, though I think larger, with thoſe found ſome time ſince in the ſame vicinity, and afford a ſpectacle truly aſtoniſhing; they appear little decayed by the lapſe of time, and their proper places, and names of the ſeveral parts of the ſkeleton, could, I preſume, eaſily be diſcovered by a perſon poſſeſſing your knowledge in anatomical ſcience.—They are, however, not as yet entirely procured, though great exertions have been made, and are ſtill making, to effect ſo deſirable an object—the difficulty is made much greater by the influx of the water, continually ruſhing in from the bottom and ſides to the hollow already made—there are among the bones found, ſeveral of the legs, ſome of the back bones, ſeveral ribs, and the upper part of the head,* &c.—one bone of the thigh meaſures more than forty inches in circumference round the joint, and thirty-ſix inches on the cylindrical part of the bone, and is nearly five feet long; the teeth are nearly ſeven inches long, and four broad—they are found white, and faſt in the jaw, without appearance of decay; the holes in the ſkull where appear to have been the noſtrils, meaſure nearly eight inches in diameter; the orifice occaſioned by the decay of marrow is, in the back bones, three inches and an half diameter; there are ſeveral others of like magnitude, and ſome bones of the foot which ſhows evident marks that it once had claws.

I muſt not forget to mention to you, my dear Sir, that eight

* The head is thought, by Dr. Graham, to have been as large as an ordinary hogſhead.

ſimilar ſkeletons have been diſcovered within eight or ten miles of the neighbouring country, and to aſſure you, that ſome of them were from fifteen to twenty feet below the ſurface of the earth, and theſe within a few years paſt. I will leave the ſubject of this prodigy for your reflections, and requeſt of you ſome remarks, to ſatisfy the public mind.

ARTICLE II.

Further Account of the Foſſil Bones in Orange and Ulſter Counties: In a Letter from Dr. JAMES G. GRAHAM, *one of the Senators of the Middle Diſtrict, to Dr.* MITCHILL; *dated Shawangunk, Sept.* 10, 1800.

DEAR SIR,

THE reſult of my inquiries and obſervations reſpecting the bones of the unknown animal found in this part of the country is as follows: Some time in 1782, ſeveral of them were diſcovered in a meadow or ſwamp about three miles ſouth of Ward's Bridge, in the town of Montgomery, now in the county of Orange, three or four feet below the ſurface, moſt of them much decayed. The next diſcovery of them was made about one mile eaſt of ſaid bridge. In this place three or four ribs were found, about eight feet below the ſurface, in a very ſound ſtate. The ſwamp here does not contain more than three or four acres, and the remaining bones of the ſkeleton probably yet remain at its bottom. About three miles eaſt of ſaid bridge ſome other bones have been found; and about ſeven miles eaſt of ſaid bridge, a tooth (one of the grinders), and ſome hair, about three inches long, of a dark dun colour, were found by Mr. Alexander Colden, four or five feet below the ſurface. About ſeven miles north-eaſterly from ſaid bridge, a vertebra has been found; and five miles weſterly from ſaid bridge, a number of bones were taken up, ſix years ago, from about five feet below the ſurface. Theſe I procured, and ſent them to Dr. Bayley, of New-York, who has, I am informed, depoſited them in Columbia College. And laſt week another ſkeleton has been diſcovered, about three miles eaſt of my houſe, in the town of Shawangunk, about ten miles north-eaſt of ſaid bridge. Theſe laſt diſcovered bones lie about ten feet from the ſurface, and are in a very ſound ſtate. Many of them have been raiſed, but ſome much

broken, eſpecially the bones of the head, which, I am perſuaded, lie entire, and in their natural order.

I have procured two bones of this laſt diſcovered ſkeleton, and ſent them to New-York, by Edward W. Laight, Eſq. for the purpoſe of having them examined by yourſelf, and other well-informed naturaliſts in the city. One of theſe I take to be a metacarpal or metatarſal bone, which indicates the animal to have been claw-footed, and, from the forms of the aſtragalus and os calcis which were among the bones ſent to Dr. Bayley, to have reſembled the foot of the bear. With reſpect to the other bone, I am at a loſs where to aſſign it a ſtation among thoſe of the ſkeleton.

Mr. Laight can inform you of many other particulars reſpecting theſe lately diſcovered bones, as he has ſeen and examined them himſelf.

Theſe large bones are uniformly found in deep wet ſwamps only, by farmers, in digging up black mould and *marl* for the purpoſe of manuring their lands. Thus a little enterprize and induſtry has enabled them to convert thoſe parts of their farms which were formerly eſteemed nuiſances, into valuable manures, and to make diſcoveries of great importance in the natural hiſtory of our country.

I have been particular in ſtating the relative ſituations and diſtances of thoſe places in which bones have been diſcovered, from a certain point, to ſhow, from the ſmall diſtrict in which many diſcoveries have been made, the great probability that theſe animals muſt have been very numerous in this part of the country: for if we compare the ſmall proportion that thoſe ſwamps, in which only they are found, bear to the reſt of the ſurface, and the very ſmall proportion that thoſe parts of ſuch ſwamps as have yet been explored, bear to the whole of ſuch ſwamps, the probable concluſion is, that they muſt once have exiſted here in great numbers. And why Providence ſhould have deſtroyed an animal or ſpecies it once thought proper to create, is a matter of curious inquiry and difficult ſolution. If, however, they were voracious, it muſt appear happy for the human race that they are extinct, by whatever means.

The hair above-mentioned ſeems to prove that it was not the elephant, or, if it was, that it muſt have been of a ſpecies or variety widely different from any known at preſent. With ſincere wiſhes for your proſperity,

I am your friend,

JAMES G. GRAHAM.

ARTICLE III.

On the SUBMERSION *of* SWALLOWS: *In a Letter to the Editor of the (New-York) Monthly Magazine.*

THE ſubſtance of what is contained in the encloſed letter, was related by the writer, in an accidental converſation on the diſappearance of Swallows. As he had preſerved a memorandum of the facts, and the utmoſt reliance could be placed on the accuracy of the ſtatement, I thought it of too much importance, in relation to a much agitated queſtion in natural hiſtory, to be withheld from the public. More particularly as, from its coincidence in time, it may ſerve to confirm a ſimilar fact, ſtated in the Medical Repoſitory, vol. ii. p. 178, firſt edition, as obſerved by Mr. Peter Cole, in this city; the truth of which is queſtioned by an anonymous writer in the 3d vol. p. 241, of the ſame work, who regards the opinion of the ſubmerſion of ſwallows as exploded. Mr. Pollock has obligingly complied with my requeſt to make the fact known, by ſending me the encloſed, with liberty to inſert it, with his name, in your uſeful Magazine. That the ſwallows could deſcend, in ſpite of their ſpecific levity, to the bottom of ſo deep and rapid a river as the Hudſon, or remain there during the winter, is not, perhaps, to be ſuppoſed.* Yet the fact of their *ſubmerſion*, after the teſtimony of Mr. P. and Mr. S. men of undoubted veracity, cannot be queſtioned. Their continuance in a torpid ſtate, and re-appearance, are different queſtions, which remain to be decided. The apparent impoſſibility of their exiſtence under water, ariſing from their peculiar organization, ſhould make us very doubtful, but not abſolutely to reject the utter poſſibility of the fact. For "natural hiſtory," ſays Kalm, who, with the reſt of the Swediſh naturaliſts, defends the *hybernation* of ſwallows, in lakes, ponds, marſhes, and caverns, "as all other hiſtories, depends not always on the intrinſic degree of probability, but upon facts founded on the teſtimony of people of noted veracity." Reaſonings and conjectures on the fact here ſtated, I leave to naturaliſts. It is to be hoped, that it may not be thought unworthy of the notice of the learned, candid, and ingenious Dr. Barton, who has already beſtowed ſo much attention on the ſubject. W. JOHNSON.

* The houſe of Mr. Pollock is ſituated near the margin of the Hudſon, about two hundred yards from the Battery. The river is about a mile and an half wide, and from ſeven to nine fathoms deep, and runs with a ſtrong and rapid tide. Mr. P. does not recollect the ſpecies of ſwallow which then diſappeared. The Barn Swallow (*Hirundo ruſtica*), Chimney Swallow (*Hirundo pelaſgia*), the Sand or Bank Martin (*Hirundo riparia*), and the Purple Martin (*Hirundo purpurea*), all frequent and build their habitations in the city and its neighbourhood.

"ON the afternoon of the 24th of Auguſt, 1798, I was ſitting in my parlour which looks towards the North River, about fifty feet from the bank, in company with our mutual friend Mr. Jacob Sebor. Our attention was attracted by numerous flights of birds, which appeared to come acroſs the town from the eaſtward, and deſcend immediately into the river. So ſingular an appearance excited our particular obſervation. We went out and ſtood cloſe to the bank, and then perceived that what we at firſt imagined to be black-birds, were actually ſwallows; and that, as ſoon as the various flocks had cleared the houſe, and got directly over the river, they plunged into the water and diſappeared. This was not confined to the vicinity of the place where we ſtood, but was the caſe as far as the eye could reach, up and down the river, and continued, without ceſſation, for nearly two hours, when the cloſing of the evening prevented our further obſervation.

"Aware of the importance of affording any additional information on this long diſputed queſtion in the natural hiſtory of the ſwallow, I procured a teleſcope, and watched attentively many of the flocks from their firſt appearance until their immerſion, continuing my eye fixed upon the ſpot long enough to be fully convinced that not one of the birds returned to the ſurface again. Indeed, one flock of about two hundred birds plunged into the water within thirty yards of us, and inſtantly diſappeared, without the leaſt appearance of oppoſition that might be expected to ariſe from their natural buoyancy, and, at the ſame time, the evening was ſo ſerene, and the river ſo unruffled, that no deception of our ſight could poſſibly have occurred.

"When the birds firſt came in view, after croſſing the town, their flight was eaſy and natural; but when they deſcended nearly to the water, they appeared much agitated and diſtreſſed, flying in a confuſed manner againſt each other, as if the love of life, common to all animals, impelled them to revolt againſt this law of nature impoſed upon their ſpecies.

"As ſome time has elapſed ſince the above mentioned facts occurred, I thought it proper, before I gave you Mr. Sebor's name, as having been a witneſs to them, to conſult his recollection on the ſubject, and I have pleaſure in aſſuring you he diſtinctly remembers every circumſtance I have recited, and of which I made a memorandum at the time.

"It may be worthy of remark, that as far as my obſervation went, the ſwallows totally diſappeared on the 24th of Auguſt, 1798; for, during the remainder of that year, I did not ſee one. "H. POLLOCK.

"*New-York, 18th July,* 1800."